AN INAUGURAL DISSERTATION:
BEING AN
ATTEMPT
TO DISPROVE THE DOCTRINE OF THE
PUTREFACTION OF THE BLOOD
OF
LIVING ANIMALS.

SUBMITTED TO THE EXAMINATION OF THE
REV. JOHN EWING, S. T. P. PROVOST;
THE TRUSTEES, AND MEDICAL PROFESSORS OF THE
UNIVERSITY OF PENNSYLVANIA,
FOR THE DEGREE OF DOCTOR OF MEDICINE;
On the 8th. day of May, A. D. 1793.

By ADAM SEYBERT, OF PHILADELPHIA; k
Honorary Member of the Philadelphia, and Member of the
American, Medical Societies.

> "FOR THE LIFE OF THE FLESH IS IN THE BLOOD."
> *Leviticus.*

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BRITISH MUSEUM

TO THE REVEREND

JUSTUS HENRY CHRISTIAN HELMUTH,
D. D.
PROFESSOR of the GERMAN LANGUAGE in the
UNIVERSITY of *PENNSYLVANIA*, &c. &c. &c.

AS a ſmall tribute of gratitude for his care and attention, in directing and ſuperintending my ſtudies, whilſt under his tuition, at the Univerſity,

THIS DISSERTATION IS,

Very reſpectfully inſcribed,

By his obliged friend,

A. SEYBERT.

To CASPAR WISTAR, M. D.

ADJUNCT PROFESSOR OF ANATOMY, SURGERY, AND MIDWIFERY IN THE *UNIVERSITY* OF *PENNSYLVANIA*.

SIR,

IN addreſſing this Inaugural Diſſertation to you, every body that knows you will readily admit the peculiar propriety of the tribute. Your juſtly acknowledged abilities leave no room to doubt that I have an intereſt in making this offering. But, whilſt I candidly admit this, I feel and confeſs profound obligations of gratitude to a beloved maſter, for his very beneficial inſtructions, and his many courteſies and attentions. To whom can the following production, with more evident propriety, be inſcribed, than to him who planted the Seed of Knowledge, and, with anxious ſolicitude, ſuperintended the growth ?—To you, Sir, I am indebted for whatever progreſs I may have made in Medical Science; and I am ambitious, that the firſt fruits of my labors ſhould be preſented to the world under the adorning ſanction of your patronage.

The cordiality of your friendſhip has naturally inſpired me with ſentiments of eſteem and attachment, which it will ever be my pride, as it is my duty and intereſt, to cheriſh.

Believe me, Sir, nothing can ever obliterate from my mind the mingled ſentiments of gratitude and eſteem with which I remain

Your obliged friend and Pupil,

A. SEYBERT.

INTRODUCTION.

THE Opinion, that the blood became putrid in many Diſeaſes, is of ancient date. It has been embraced, with various modifications by moſt of the ſects in Medicine. It particularly engaged the attention and belief of the learned Boerhaave, who publicly taught it in the celebrated ſchool of Leyden. Under the impoſing authority of his name, and by means of his induſtry, it was diffuſed throughout almoſt the whole globe. It became the theme of the vulgar; and at this day has many great and illuſtrious names to ſupport it.

There have been, however, at different times, a few Medical Philoſophers, who, although they adopted it in ſome inſtances, ſeem to have had doubts reſpecting the truth of the doctrine, and to have rejected it in their explanations of many diſeaſes. In this claſs may be reckoned the celebrated Hoffmann, who explained the phænomena of many diſorders without any regard to the ſtate of the blood: He was followed in this by the great Cullen, who, in his ſyſtem, attends leſs to the ſtate of the fluids in diſeaſes than any of his predeceſſors. The doctrine has of late been wholly denied by the very ingenious Dr. Milman in his treatiſe on the Scurvy; and entirely rejected by the late ſagacious Dr. Brown of Edinburgh, from his Syſtem of Medicine. Many phyſicians appear to have adopted the ſentiments of theſe gentlemen; ſo that at preſent there is a diverſity of opinion among medical philoſophers reſpecting the Putrefaction of the Blood of *Living Animals*.

On examining the ſubject, I found that it had never been put to the teſt of fair experiment; but, that ſpeculative reaſoning (too often deluſive at beſt) and a few indeciſive facts, were the chief ſupports of the argument on both ſides. With theſe data, the reſult was neither ſatisfactory nor convincing. I then determined to contribute my mite towards inveſtigating the matter by experiment. In the proſecution of this attempt, my attention has been directed to the advancement of Science; and, in the detailing of the Experiments which I have made with this view, truth ſhall be my polar ſtar.

I have been induced from reflection and experiment to adopt a poſitive belief on this ſubject, and to deny the truth of the doctrine which I have juſt noticed. I am, therefore, neceſſarily led into an oppoſition to the opinions of many celebrated men. But, I truſt, I have obſerved a decent regard and veneration for thoſe from whom I diſſent, without being at all depreſſed by the weight of their authority. Having viewed nature *attentively*, I ſhall endeavour to communicate *accurately* what I have obſerved. I will not ſacrifice truth to the luſtre of great names, but, with confidence adopt the ſentiment of Cicero—"Mea fuit ſemper hæc in hac re voluntas et ſententia, quemvis ut hoc mallem de iis, qui eſſent idonei, ſuſcipere quam me: me, ut mallem quam neminem."

INAUGURAL DISSERTATION.

IT will be proper, before I enter upon the conſideration of the Putrefaction of the Blood of living Animals, to take a brief view of *Putrefaction in general*; for, this is, indeed, the only method by which we can be prepared for an examination of the circumſtances neceſſary to induce that ſtate of the blood in living animals.

Though Chemiſtry is much improved, and numerous diſcoveries are made almoſt every day, by different philoſophers, *Putrefaction* is, at this moment conſidered as the ſame difficult ſubject, that it was in the days of the celebrated *Lord Bacon*.

This laſt ſtage of Fermentation, in the days of Stahl, was ſuppoſed to be a mere conſequence of the *vinous* and *acetous* ſtages; but, modern diſcoveries teach us the contrary; for we find, that while ſome ſubſtances undergo only the laſt ſtage, others ſuffer the three ſucceſſive changes in a regular manner: thus mucilages, &c. become acid without undergoing the vinous fermentation, and the glutinous matter of vegetables will putrefy before it undergoes either of the other changes.

Obſervation, the grand parent of diſcovery, has taught us, that no ſubſtance is capable of undergoing a change by the putrefactive fermentation, except it be *animal* or *vegetable*; and that the numerous claſſes of the productions of nature, comprehended under the title of the Mineral Kingdom, are excluded. It is alſo an opinion, generally eſtabliſhed, and proved by experiment, that the fluid and ſofter parts of thoſe bodies, putrefy much ſooner than the harder and more ſolid parts. It has likewiſe been obſerved, "That the fleſh of younger animals is ſomewhat more prone to putridity, than that of older animals."*

Animal and vegetable matters cannot putrefy in every ſituation or condition in which they may exiſt; for it is neceſſary that a living animal or vegetable ſhould undergo a conſiderable change, before it can be rendered capable thereof: It muſt even be deprived of life, or the vital principle. No one has ever ſeen an entire animal or vegetable putrefy whilſt alive; and Beccher, on this ſubject, beautifully obſerves: "Cauſa putrefactionis primaria defectus ſpiritus vitalis balſamini eſt." And, indeed, in all reſearches into thoſe kingdoms which are the ſubjects of *fermentation*, it is of ſo great conſequence to keep this univerſal actuating principle in view, that by neglecting it, we may commit great miſtakes, and look to other cauſes than the true ones for its palpable effects; inſomuch that the learned Chaptal, when regretting the imperfect ſucceſs which Chemiſtry has met with, in the analyſis of animal matters, cannot help obſerving it. "All (ſays he) have miſtaken or overlooked that principle of life which inceſſantly acts upon the ſolids and fluids, modifies, without ceaſing, the impreſſion of external objects; impedes the degenerations which depend on the conſtitution itſelf; and preſents to us phænomena which chemiſtry never could have known or predicted by attending to the invariable laws obſerved in inanimate bodies."*

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* Medical Commentaries, Vol. II. p. 142.

The preſence of that inviſible elaſtic fluid, which we term *vital air*, is ſo neceſſary to putrefaction, that a body cannot putrefy without being in contact with it; and may be preſerved ſound and pure for years if the communication between them be deſtroyed. It is a well known fact that a body will not putrefy *in vacuo*: This has been noticed by an ingenious author in the following words: "How much the air contributes to putrefaction, is evident hence, that bodies buried deep under the earth, or in water, out of the reach of air, ſhall remain for ages entire; which yet, being expoſed to the open air, ſhall ſoon rot and moulder away."†

It appears that too great a degree of moiſture, or a total want of it, retards the proceſs of putrefaction. In order, therefore, that a body may putrefy, it is neceſſary that it be only duly moiſtened. Thus it happens, that after an animal or vegetable ſubſtance has been made perfectly dry, it may be preſerved, in that ſtate, for many years after. It has been obſerved by the immortal Becher, that too great a degree of moiſture prevents putrefaction: Theſe are his words; "Nimia quoque humiditas a putrefactione impedit, prout nimius calor; nam corpora in aqua potius gradatim conſumi quam putreſcere, ſi nova ſemper affluens ſit, experientia docet: unde longo tempore integra interdum ſubmerſa prorſus a putrefactione immunia vidimus; adeo ut nobis aliquando ſpeculatio occurreret, tractando tali modo cadavera anatomiæ ſubjicienda, quo diutius a fœtore et putrefactione immunia forent."*

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* Chaptal's Chemiſtry, Vol. III. p. 280.

† Frewen's Phyſiologia, p. 128.

That *all* enlivening principle, *heat*, which, in a certain degree, is neceſſary to life, is no leſs neceſſary to the bringing on of the diſſolution of a body. Temperature has been found to have great influence in promoting and retarding putrefaction.

I have now mentioned the moſt eſſential circumſtances neceſſary to promote the inception of putrefaction; to which we may ſubjoin *Reſt*; for bodies do not putrefy while in continual motion. In proportion as theſe circumſtances take place the proceſs will advance with greater or leſſer rapidity.

Certain ſubſtances, as well known to the vulgar as to the philoſopher, by the name of Ferments, when added to a fermentable maſs, are found to haſten the proceſs in a manner truly aſtoniſhing; though both the peaſant and the philoſopher ſtand on an equal footing with regard to a knowledge of the principle by which their application produces a ſpecific operation. "We are told indeed (ſays the ingenious Mr. Henry) that a vinous ferment induces the vinous, that a ferment of an acetous kind brings on the acetous fermentation, and a putrid one, that fermentation which ends in putrefaction. But we receive no more information, relative to the manner in which they produce thoſe effects, than we do with regard to fermentation itſelf."†

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* Phyſ. Sub. lib. 1. ſ. 5. cap. 1. p. 277.

† Mancheſter Memoirs, Vol. II. p. 259.

I will now proceed to examine, whether, in becoming putrid, a body undergoes any conſiderable change; and, whether it be poſſible to remove putreſcency after it has taken place.

The very meaning of the word *putrefaction* conveys the idea that an eſſential change muſt have taken place in any ſubſtance which has acquired a putrid ſtate.

Putrefaction reduces both animals and vegetables to the ſame principles, for, it cauſes an entire and complete decompoſition of them, inſomuch that it is difficult, and indeed impoſſible, to diſtinguiſh between a putrefied maſs of animal and one of vegetable matter. The former characteriſtics of each are at an end. Colour, texture, and every ſenſible quality of the body, are thereby deſtroyed. It reduces the animate part of the Creation to an indiſcriminate level with inanimate matter. There is abundance of truth in the obſervation, that bodies, in this proceſs, undergo a new combination, as well as ſeparation of their conſtituent parts. Putrefaction cauſes the ſweeteſt ſubſtances to become the moſt offenſive and diſagreeable to the ſmell; and, inſtead of a vegetable acid, at length produces a volatile alkali—bodies poſſeſſing very oppoſite principles.

Many philoſophers have gone ſo far as to ſay, that, by certain chemical proceſſes, they were able not only to render putrid ſubſtances ſweet again, but reſtore them to the condition they were in, previouſly to their undergoing this peculiar change. This, according to them, is to be effected by ſurrounding the putrefied body with an atmoſphere of fixed air; to the loſs of which principle, an enlightened philoſopher wholly attributed the changes produced in a body by putrefaction; though the opinion has been ingeniouſly refuted, and therefore needs not much conſideration in this place*.

It is true, that by ſurrounding a putrefied body with fixed air, we ſhall prevent the advancement of the proceſs; yet the fixed air has no ſpecific operation in this reſpect, for other ſubſtances poſſeſs a ſimilar property. After I had ſuffered ſeveral pieces of highly putrid beef to remain completely covered with freſh pump water for ſeveral hours, and then waſhed them frequently in water ſeveral times renewed, I found, that by this operation the water gained a highly offenſive and putrid ſmell, and that the meat had loſt a great degree of its own; but its ſolidity was not in the leaſt reſtored. It is a common and well-known fact in domeſtic œconomy, that meat, which has become tainted, is very frequently waſhed in freſh water before it is cooked; and the reaſon aſſigned for this proceſs is, that the meat is thereby rendered ſweeter, Thus do houſe-keepers reaſon from a knowledge of the fact without any theory to bias them.

Hence it appears very clear, that to explain this fact no ſpecific operation is neceſſary to be recurred to, ſince water, free from the combination of fixed air, had effects ſimilar to thoſe produced by fixed air itſelf. Mr. Chaptal, in my opinion, explains the operation of this, and of the other ſubſtances which are ſaid to have the like effect, upon very ſimple and philoſophical principles. When ſpeaking of the neceſſity of the preſence of vital air to putrefaction, he ſays, "We ſhall obſerve, on this ſubject, that the effects obſerved in fleſh expoſed to the Carbonic acid, Nitrogene gas, &c. are referable to a ſimilar cauſe; and it appears to me that it is without ſufficient proof that a concluſion has been drawn, that theſe ſame gaſes, internally taken, ought to be conſidered as antiſeptic; becauſe, in the caſes we have mentioned, they act only by defending the bodies they ſurround from the contact of vital air, which is the principle of putrefaction *."

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* Medical Commentaries, Vol. II. p. 150.

To me it appears plain, that, ſo great is the chemical change and decompoſition which a putrid body has undergone, that no means can remedy it. On this point Fontana truly obſerves, "We do not know any power, nature herſelf does not diſcloſe any, that can recompoſe an organ that is deſtroyed, and entirely decompoſed by putrefaction, or by the concuſſions of external bodies. This is what has never yet either been accompliſhed or ſeen. We have, therefore, every poſſible reaſon not only to believe an animal that is reduced to this ſtate dead, but likewiſe to believe it dead for ever †."

From the above conſideration of putrefaction, as going on out of the body, it appears, that a certain degree of heat, the preſence of vital air, a certain degree of moiſture, together with reſt, are indiſpenſably neceſſary to putrefaction; and that without their preſence no body can putrefy. We alſo know, that by adding certain ſubſtances to bodies which are to undergo the change, the proceſs is haſtened in a manner ſurpriſing to every one who has ever had occaſion to notice it.

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* Chaptal's Chemiſtry, Vol. III. p. 398.

† Fontana on Poiſons, Vol. I. p. 406.

Having conſidered the circumſtances in which fermentation, in general, takes place, as it is conceivable that if the above circumſtances be preſent in the living animal, the proceſs might readily go on in it—let us examine if theſe neceſſary requiſites be preſent, ſo as to act in a due and proper manner upon that vital fluid, *the blood.*

Several queſtions occur here.—Is there any difference between dead and living matter?—Is there not in living animals a poſitive power of reſiſting putrefaction?—Whether the nice and inexplicable operation which is employed in converting the dull maſs of the motionleſs creation into the peculiar condition of organic ſenſibility, acts to no purpoſe?—In fact, whether the principal characteriſtic of death, has not been determined by phyſiologiſts to be the beginning of putrefaction in the body?

The principle we denominate *vital,* with whoſe effects we begin to be acquainted, though its nature and origin will perhaps ever be hidden from us, is found to be the chief impediment to the putrefaction of a living animal. As ſoon as the animal is deprived of it, putrefaction is the inevitable conſequence, except the above precautions be well obſerved. This may be fairly demonſtrated. All the circumſtances neceſſary to putrefaction take place with reſpect to the external ſurface of our bodies, yet they do not putrefy while alive; but if life be by any means deſtroyed, they will as readily undergo the proceſs as other inanimate matter.

It is to be obſerved, that by the living principle is underſtood that power which in an animal actuates its whole ſyſtem, or from which is derived ſenſation, motion, and the other qualities of life. It is the cauſe of the preſervation of the body from diſſolution, and is capable of exiſting, for ſometime, under a ſuſpenſion of all its actions *.

We will now proceed to conſider the preſence of *heat*, pure *air*, *moiſture*, and *reſt*, with regard to the blood; in order to determine whether we might expect a putrefaction of that fluid if the principle of life, were not alone ſufficient to obviate it.

1ſt. Of Heat. Though different degrees of heat are found ſufficient to maintain life, in different animals, yet no animal while alive has ever been found devoid of a conſiderable quantity of it; indeed, ſo careful was nature in this reſpect, that ſhe has endowed man, and the inferior animals, with a power, whereby they are capable of generating heat;—a proceſs the inveſtigation of which has of late much engaged the attention of philoſophers, and upon which conſiderable light has been thrown. It is probable that the blood of every living animal contains a degree of heat ſufficient to ſupport the proceſs of putrefaction.

2dly. Pure Air. That element ſo neceſſary to our exiſtence, and which we receive into our bodies, by means of thoſe vital viſcera, the lungs, is no leſs neceſſary to the maintaining of animal life, than to the proceſs of putrefaction. A man will no more live, than a dead body will purefy, in vacuo †.

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* Gardiner's Animal Economy, p. 3.

† That air is a very active and powerful agent in putrefaction, is evident from the following fact, viz. Whilſt pus remains ſhut up in

It was for a long time ſuppoſed that elaſtic air exiſted in the blood-veſſels of living animals; but, the experiments of the ingenious Darwin clearly prove the contrary; the following is one of them. "Part of the jugular vein of a ſheep, full of blood, was included between two tight ligatures, and cut out while the animal was yet alive. It was immediately put into a glaſs of warm water, and placed in the receiver of an air pump. It ſunk at firſt to the bottom of the water, and did not riſe again, although the air was carefully exhauſted. After this, it was wiped dry, and laid on the braſs floor of the receiver. The air was again exhauſted, but there was not the leaſt viſible expanſion of the vein or its contents *."

By the experiments above referred to it is reduced to a certainty, that no air exiſts, formally in the blood, while encloſed in the blood-veſſels. And it is evident, that it was without ſufficient grounds, that philoſophers inferred, that air exiſted in the blood, while encloſed in the blood-veſſels; becauſe they perceived it in blood drawn from a vein, and placed in the receiver of an air pump; for during its expoſure it muſt have had time to abſorb air from the atmoſphere.

The celebrated Huxham was of opinion, that, "elaſtic air is probably generated in the arterial and venous ſyſtems,

in a perfectly cloſe cavity it will keep ſweet and inodorous, but on expoſure to the atmoſphere, it contracts in a very ſhort time a putrid ſmell; the ſame circumſtance takes place with regard to extravaſated blood.

* Medical Commentaries, Vol. VI. p. 35.

tems, in putrid fevers *," though he has no experiments to prove it. Let us then examine, whether air can exiſt in the blood-veſſels of living animals.

With this view the ingenious Luzuriaga tried many experiments on living dogs. He injected ſeveral different kinds of air into the blood-veſſels, and in every inſtance the animals were killed, in a very ſhort time. I will only mention that he twice injected inflammable air; once phlogiſticated air; once fixed air; once nitrous air. I will ſtate the particular circumſtances that happened in the inſtance, when dephlogiſticated or pure air, which is the chief agent of putrefaction, was injected.—It was forced into the jugular vein of a dog, and in three minutes he died. On diſſection, the blood appeared of a lively red colour, and frothy, but not grumous nor coagulated †.

Thus it appears, that air does not, and cannot exiſt, formally, in the blood-veſſels of a living animal; a circumſtance which refutes the opinion, that the blood is capable of undergoing the proceſs of of putrefaction, during the life of the animal.

3dly. Moiſture. No perſon in his ſenſes dares to deny the preſence of this third circumſtance, as eſſential to putrefaction, and therefore it needs no further conſideration.

4thly. We have mentioned that it was neceſſary for a body, which is to undergo the putrefactive proceſs, to be at reſt. The continual motion of the blood of living animals,

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[*] Medical Obſervations, Vol. III. p. 36.

[†] Luzuriaga's Inaug. Diſſertat. Eden. 1786, p. 26.

animals, muſt certainly be a great impediment to the proceſs; and indeed motion has been found to be a very effectual means of preventing it; even a briſk wind has been known to retard it *.

We have found that the life of the animal, the want of air in the blood-veſſels, and its circulatory motion, were great impediments to the putrefaction of the blood; we ſhall now proceed to conſider, whether the principle of the animal life is contained in the blood.

The ſituation of that principle, to which we owe our exiſtence as living beings, is at preſent much diſputed. While ſome Phyſiologiſts will have it to exiſt wholly in the nervous ſyſtem, in the form of a ſubtle fluid, whoſe preſence the beſt microſcopes have not been able to diſcover; there are others, whoſe authority is by no means inferior, who ſtrenuouſly oppoſe the doctrine, and perſiſt that blood partakes of it alſo. In this latter claſs we may rank the celebrated Hunter and Fontana.

That the life of the animal exiſts in the blood, is an opinion of as ancient a date as Holy Writ itſelf: It was the favourite ſentiment of many ancient philoſophers; and the great Harvey, to whom we are ſo much indebted, ſays, "the blood is the primum vivens, and the ultimum moriens, of the animal."

This hypotheſis was for ſometime ſunk into oblivion, but was again revived and placed on a firm baſis by the able Mr. John Hunter of London; who ſupports the doctrine by

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* Medical Commentaries, Vol. II. p. 146.

by reaſoning truly philoſophical, and by experiments, in my opinion, incontrovertibly convincing. It would be needleſs for me to repeat all the ingenious arguments which are made uſe of by him on this occaſion, as they may be eaſily ſeen, by referring to the Encyclopœdia, Vol. III. p. 313.

His 5th argument alone would almoſt ſuffice to convince me of the truth of his opinion. "The blood (ſays he) preſerves life in the different parts of the body. When the nerves going to a part are tied or cut, the part becomes paralytic, and looſes all power of motion; but it does not mortify; if the artery be cut, the part dies, and mortification enſues. What keeps it alive in the firſt place? Mr. Hunter believes it is the living principle which alone can keep it alive; and he thinks that this phenomenon is inexplicable on any other ſuppoſition, than that life is ſupported by the blood"*.

This doctrine is every day gaining ground; and appears to be further ſupported by the opinions and experiments, of the celebrated Fontana; who obſerves, "What may lead one to ſuſpect, that a very active and volatile principle does really exiſt in the blood, is, that the vipers venom prevents its coagulation when it is drawn from the veſſels, and on the contrary, produces it in the veſſels themſelves. One would ſuppoſe (ſays he) in the firſt caſe, that ſomething had flown off from the blood, which exiſts in it when it is encloſed by the veſſels" †.

"My experiments on animals (continues this laſt mentioned enlighted philoſopher) in which the nerves were bit by vipers, ſhew that the venom is a ſubſtance perfectly innocent to theſe organs, that it does not occaſion in them any ſenſible change, and that they are not even a means or vehicle of conveying it to the animal. In a word, it appears that the nervous ſyſtem does not concur more to the production of the diſeaſes of the venom, than does the tendon, or any other inſenſible part of the animal: on the other hand, all the experiments on the blood, the injection of venom into the veſſels, and ſo ſoon conſtantly evince that the action of the venom of the viper is on the blood itſelf. This fluid is alone changed by the venom, and this fluid conveys the venom to the animal, and diſtributes it to its whole body. The action of the venom, and its effects on the blood, are almoſt inſtantaneous."*

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* Encyclopœdia Loc. citat.

† Fontana on Poiſons, Vol. II. p. 135.

His experiments, in my opinion, allow this inference, that ſince life was not deſtroyed by the immediate application of the poiſon to the bare nerves, and that the loſs of it was almoſt inſtantaneous when the poiſon was applied to the blood; there exiſts ſome quality in the blood, that does not exiſt in the nerves; and as death can only be produced by deſtroying the vital principle, this principle muſt conſequently exiſt in the blood, and in a ſtate different from that in which it exiſts in the nervous ſyſtem.

I will now proceed to give a connected view of the reaſons, which together with the experiments hereafter to be mentioned, induce me to deny the putrefaction of the blood to have ever taken place, and which confirm me in believing, that no ſuch change can happen in the blood of living animals.

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* Fontana on Poiſons, Vol. I. p. 396.

1ſt. We have ſeen that the proceſs of putrefaction cannot take place in the animal or vegetable kingdoms, until after the animal or plant be deprived of life. I think we have given ſome proof of the blood's containing the principle of Animal Life, of which (whether it be convincing or not) the candid will judge. When life is once perfectly deſtroyed, there are no means, at our command, by which it can be again reſtored: therefore, we muſt infer, that all putrid caſes are neceſſarily mortal, or a putrefaction of the blood cannot take place.

2dly. From viewing the proceſs of mortification, as going on in the ſolid parts of animals—for the death of the part *invariably* precedes the ſymptoms of putreſcency, as is evidenced by the want of ſenſibility in the part.

3dly. There is no vital air in the blood-veſſels, and we know putrefaction cannot go on without it.

4thly. The blood in the living animal is in continual motion—a ſmall degree of motion has been known to impede the proceſs.

5thly. The doctrine of ferments. For as long as a drop of this putrid blood remained in the veſſels, no particle could exiſt in them, without being changed by it; therefore it is a neceſſary conſequence, that in every caſe, where putrefaction is ſuppoſed to have taken place in the blood, every drop of that fluid ſhould be drawn from the veſſels, and the veſſels ſhould undergo a perfect cleanſing before healthy blood could exiſt in them; conſequently, if the veſſels are not cleanſed, the proceſs muſt neceſſarily continue for ever, and in attempting to cleanſe them, would not death be the inevitable conſequence? *

6thly. We are as yet unacquainted with any means by which we are able to recover a body, when once putrid; for, according to a chemical axiom, it has undergone a perfect decompoſition in the proceſs; and, certainly, without obviating the effects of putrefaction on the blood, and reducing it to its former ſtate, the animal cannot exiſt for any length of time.

7thly. In every caſe of putrefaction a great quantity of elaſtic air is evolved, and no author (as far as I know) has made mention of the preſence of air in the blood-veſſels of thoſe who have died of diſeaſes, which were ſuppoſed to belong to the putrid claſs, and whoſe bodies were diſſected immediately after death. It has been fairly proved that air, on being injected into the veſſels, kills in a very ſhort time †.

8thly. According to the ideas of the ſupporters of the putrefaction of the blood in diſeaſes, the proceſs muſt be

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* The difficulty of reſtraining putrefaction after it has taken place, and of deſtroying the putrid ferment in veſſels that have contained putrid ſubſtances, is well known to the brewers. When their liquor becomes putrid, or as they term it, "foxed," the veſſels which contained it become ſo contaminated, that they will excite the ſame fermentation in any freſh liquor that may be put into them, even after they are waſhed. It is only by white-waſhing or ſcouring them with lime that they can be thoroughly cleanſed. I have heard of a brewer, who was ſo much embarraſſed by a putrid ferment in his veſſels, that he was about giving up his buſineſs, when he fortunately learned the uſe of lime.

† Vide ante Page 19.

different in Typhus, &c. from what it is in ſcurvy; ſince, in the former caſe, bark and wine, together with other ſtimuli, are the only true remedies; whereas, according to them, theſe remedies are found to be of little or no ſervice in the latter, and freſh vegetables are its only remedies. For the blood, they ſay, is putrid in both inſtances; and ſince putrefaction going on in the ſame ſubſtance would appear to be always the ſame, we might, with propriety ſuppoſe, that, what retards and corrects it in the one inſtance, would do it in the other. Who has thought of exhibiting ſuch vegetables, in typhus, yellow fever, &c. and neglecting thoſe powerful remedies, bark and wine?

9thly. It is incompatible with ſound reaſoning to ſuppoſe that putrefaction of the blood takes place in the plague, &c. ſince we find that perſons ſometimes fall down dead, immediately on being expoſed to the effluvia, that have been ſaid to give origin to this and other diſeaſes of the ſame claſs, as the porters, who opened bales of goods in the lazarettos of Marſeilles. Mead relates, that, "Upon opening one of the bales of wool in a field, two Turks employed in the work were immediately killed, and ſome birds which happened to fly over the place, dropped down dead *." How is it poſſible that the maſs of blood, or any part of it, could become in the leaſt degree putrid in thoſe inſtances? For the ſpace of time, before death was produced, was too ſhort to allow of it. Since the poiſon kills ſo quickly, can we with reaſon ſuppoſe that an animal can live while the fountain of life is impregnated with it? If a ſmall portion of ſuch effluvia produces diſeaſe, when its action

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[*] Mead's Works, p. 198.

may, excluſively, be ſuppoſed to be merely on the external ſurface of the body, Is there not ſtrong ground for believing that inſtant death would follow if the blood were in the leaſt tainted by it?

10thly. Experiments ſeem to prove, that the blood is peculiarly qualified to excite motion in the heart and arteries, and that no other fluid will anſwer the ſame purpoſe.—That even milk, and other bland liquors, when injected into the veſſels of living animals, kill. We know it is an eſtabliſhed truth, that by putrefaction bodies undergo a complete decompoſition and diſſolution of their of their conſtituent parts, and are rendered totally different from what they were before—How then can we imagine that life can be kept up whilſt the blood is in a putrid ſtate in the veſſels?

For theſe reaſons I concluded that a putrefaction of the blood could not take place in the living body: but the importance of the ſubject induced me to engage in a ſeries of Experiments which might enable me, with more indiſputable certainty, to decide the queſtion, Whether the blood of living animals can be rendered putrid?

And this I ſuppoſe, if poſſible, would be aſcertained by the following circumſtances, viz.

I. By Starvation.

II. By a putrid Diet.

And, III. by injecting putrid and other ſubſtances into blood-veſſels.

1ſt. Of Starvation. When we conſider the many and various actions performed by man and other animals, we plainly ſee why a certain quantity of aliment is neceſſary to be taken by them. Phyſiology teaches, that by every action man ſuffers a loſs of the ſolids, and that by every ſecretion the blood becomes leſs in quantity.

The blood being the fountain from whence all the ſolid parts derive their ſupport, and the fluids their origin, it muſt naturally have been ſuppoſed to be affected by ſtarvation, as it is immediately prepared from the aliment we take in.

If the caſe be as above ſtated, is it not ſurpriſing that man can live for a length of time without taking any nouriſhment at all? There are inſtances related where men have lived for ſix, twelve, and even fourteen days, or longer, without receiving any aliment *.

Drink is found to be no leſs neceſſary to the maintaining of animal life than the ſolid matters of our food. The neceſſity of this article was ſuppoſed to be merely in recruiting, blunting, and correcting the acrimony of the blood which would take place if we did not uſe them. And it has been found by experiment, that life may be ſupported a much longer time by water than it could without it. Rhedi, who made experiments to aſcertain how much longer life might be ſupported by the uſe of drink, without the uſe of any other article of diet, than when the animal was totally deprived of all food, found, that fowls to whom he gave no drink lived until the ninth day; whereas

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* Manchester Memoirs, Vol. II. p. 467. and ſeq.

whereas one who was allowed water, lived more than twenty days *.

Though man may live for a conſiderable length of time, without taking in any nouriſhment, yet his ſituation is not agreeable; for, he not only ſuffers pain, but all the functions, as well bodily as mental, are in conſequence affected; his ſenſations are ſtrange, his ideas confuſed, his ſight deceptive and deranged, his countenance becomes pale and ſallow, his weight decreaſes conſiderably, though the excretions are ſparing or none at all, the reſpiration ſuffers, the action of the heart decreaſes in proportion, till at laſt no pulſation can be felt, debility by degrees overpowering life, death approaches and puts an end to his ſufferings.

If after having faſted a length of time, the perſon recover, he becomes melancholy, hyſterical, effeminate, ſtupid, &c. †

The functions performed by the fluids and ſolids of an animal, being ſo nearly connected the one with the other, have occaſioned this queſtion, Does ſtarvation operate by inducing diſeaſe in the ſolids or fluids? Moſt phyſiologiſts are in favour of an opinion, that the fluids are particularly affected, and that by ſtarvation the animal ſuffers from the quality, rather than from the deficient quantity of the blood. They ſuppoſe it to become acrid and even putrid.

To determine whether it was the blood that ſuffered, I performed the 2d. and 3d. following Experiments, and alſo

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* Mancheſter Memoirs, Vol. II. p. 575.

† Halleric Elementa Phyſiologiæ.

alſo examined the phænomena preſented by the blood of an healthy dog, the better to judge of the ſtate of that fluid in all the ſucceeding Experiments. And let me here obſerve, that the Experiments I ſhall have occaſion to relate, were made in the preſence, and with the aſſiſtance of many worthy friends, Meſſrs. Jeſſe Thomſon, John Andrews my fellow graduate, and Jacob Wagner, junior. They were witneſſes to all the phænomena; and I take this public method of acknowledging my obligations to them for their kind aſſiſtance.

EXPERIMENT I.

September 6th. 1792. An ounce of blood was drawn from one of the crural veins of an healthy dog, at 15 minutes after 2 o'clock, P.M. it was coagulated at 20 minutes after 2; ſeparation into ſerum and craſſamentum was evident at 34 minutes after 2. Serum did not change the colour of a piece of paper ſtained with a vegetable blue.*

EXPERIMENT II.

A young dog was kept for the purpoſe of ſtarvation, and the phænomena were as I ſhall now relate.

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* The vegetable blue was made uſe of as a teſt, with the view of aſcertaining whether the ſerum of an healthy animal contained any alcaline ſalt in a formal ſtate, as it is a well eſtabliſhed fact that the volatile alcali is produced by putrefaction; and the production of it is one of the characteriſtic circumſtances of that proceſs: the ſame teſt was made uſe of in the ſubſequent experiments, to aſcertain whether ſerum gained any alkaline properties by the different treatment which the ſeveral animals ſuffered.

I thought it not unneceſſary to weigh him, at different times. I ſhall therefore mention the weight, as obſerved at thoſe ſeveral periods.

July the 8th. he weighed 5¾ lb. July 29th. 7½lb.

Food was given him the laſt time, in the afternoon of the 29th. of July.

Auguſt 3d. he weighed 6½ lb.

Auguſt 12th. he weighed 5lb. At 2 minutes before 12 o'clock an ounce of blood was drawn from one of the crural veſſels. The blood flowed freely, but by misfortune an artery was opened inſtead of a vein. The colour and ſmell of the blood were quite natural, being highly florid like arterial blood. It was coagulated at 3 minutes after 12—ſeparation into ſerum and craſſamentum was evident at 13 minutes after 12. The ſerum and craſſamentum appeared perfectly natural. A piece of the blue ſtained paper was dipped into the ſerum, and no change of colour was produced. To prevent a further hæmorrhage, as he was much weakened by the bleeding, the wound was well cloſed, and a piece of ſpunge bound tightly over it. The leg became paralytic.

Auguſt 13th. the leg was much ſwolen below the ligature, and when handled, did not appear to give him any pain. The ligature was ſomewhat looſened.

Auguſt 14th. weighed 4½ lb.

Auguſt 15th. This morning at 8 o'clock I found him dead.

During the above management the dog had very few evacuations by ſtool or urine, till the two laſt days of his life, when the urine dropped continually from the urethra.

His eyes were ſeveral times examined, and no alteration in them was evident.

He did not appear to ſuffer pain till the 3d. of Auguſt, at which time he cried very much, gaped frequently, and appeared very weak; weakneſs continuing daily to increaſe till his death.

I did not perceive that reſpiration was much affected; but the action of the heart became ſo feeble, that I could ſcarcely perceive its beating for ſome days previous to his death, though I applied my hand to the part where its ſtroke is generally felt.

The abdomen was much contracted, and the fore-part of it was drawn up to the ſpine.

The body was examined immediately on finding him dead, when appearances were as follow:

The ſtomach contained a conſiderable quantity of a whitiſh fluid, its texture was perfectly natural. The inteſtines were of a greeniſh caſt. The other viſcera appeared in a ſound and natural ſtate.

At 39 minutes after 8 o'clock, blood was caught in a tumbler from an opening made into the heart; colour and ſmell natural. It was coagulated at 45 minutes after 8. It began to ſeparate into ſerum and craſſamentum at 51 minutes after 8. Coagulum and ſerum perfectly natural. Serum did not change the paper ſtained blue.

EXPERIMENT III.

April 10th 1793, a dog was kept for the purpoſe of *ſtarvation.*

April 20th. Blood was drawn from him at 15 minutes paſt 3 o'clock. The colour and ſmell were natural—it coagulated at 20 minutes after 3—and ſeparated into ſerum and craſſamentum at 35 minutes after 3—The ſerum was not in the leaſt acrid to the taſte.

April 26th. Blood was drawn at 33 minutes paſt 3 o'clock—The colour and ſmell were natural—It coagulated at 38 minutes paſt 3—and ſeparated into ſerum and craſſamentum at 53 minutes paſt 3—The coagulum and ſerum were natural—The ſerum was not in the leaſt acrid to the taſte.

2dly. *Of Putrid Diet.* The neceſſity of our taking aliment being fully eſtabliſhed, Nature, with her uſual wiſdom, has given man and the other animals an appetite for it. She has alſo provided different kinds of aliment in the different climates, in a proper quantity, ſo that each might have what was moſt ſuitable and agreeable to his particular condition.

To this purpoſe the appetites of the inhabitants of the different regions vary; while ſome prefer a vegetable, others according to the ſituation, with more pleaſure and benefit to their health, derive their chief ſupport from the ſame claſs of beings to which they themſelves belong, though of different genera and ſpecies.

Animals are ſtyled herbivorous, carnivorous, and omnivorous, from the particular kind of aliment they make uſe of: Thus ſheep live wholly on a vegetable diet, while the wolf and other animals of the ſame ſpecies are found to ſupport themſelves beſt by a diet which is entirely animal; and man, from the peculiar ſtructure of his teeth, appears to be deſtined to take in all kinds of alimentary matter, and thus he is found to live beſt on a diet, compoſed of animal and vegetable ſubſtances. Experiments would ſeem to prove that an animal, who is naturally herbivorous, may be made to live entirely on fleſh, although not ſo conveniently. A mixed diet, as already mentioned, beſt ſuits the appetite and peculiar ſtate of man, but there are inſtances where he entirely lives on vegetables; and on the contrary, he has, in other ſituations, been found to live wholly on the inferior animals. While the mixed diet renders him placid and fit for every purpoſe, for which nature formed him, one entirely animal renders him ferocious as the brute, and that altogether vegetable renders him weak and feeble.

Among civilized nations, aliment, before it is eaten, always undergoes ſome kind of preparation, whereby it is rendered more or leſs fit to be ſubject of digeſtion.

Aliment, after being received into the ſtomach, remains there for ſome time, and undergoes a conſiderable change, before it quits that viſcus; for it thereby becomes fitted for the forming of the chyle, from which the blood itſelf is immediately prepared.

The ſtate of the chyle and conſequently that of the blood, is by many ſuppoſed to be much influenced by the aliment

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we take; thus a putrid and bad diet, is ſuppoſed to produce vitiated and putreſcent chyle, and conſequently in their opinion the blood is tainted in the ſame manner. This opinion probably took its riſe from the fleſh of certain animals taſting ſimilar to the food they eat; thus ſea-fowls, we know, have a fiſhy taſte; pidgeons who have fed on poke-berries for ſometime have their fleſh tinged with the colouring matter of that vegetable; and the fleſh of the pheaſant who has lived upon laurel-berries is capable of communicating the deadly effects of that active poiſon to the human ſyſtem. Theſe, and perhaps other like circumſtances, were the means of giving riſe to the opinion, that the blood of animals muſt be affected differently by different articles of diet, and that corrupted or putrid food would be the means of producing a putrefaction of the blood. As long ſince as the days of Hippocrates, phyſicians have been of opinion that water and corrupted meat were the true cauſes of putrid diſeaſes. In this manner was the plague, ſcurvy, &c. thought to have been produced; but the ſentiment of many moderns is, "That ſcurvy ariſes from the want of a due quantity of alimentary matter in the food of thoſe who are afflicted with the diſeaſe." This opinion is ſupported by the two caſes of ſcurvy related in the Medical Tranſactions by Dr. Milman, and alſo by Dr. Stark's Dietetic Experiments *.

To convince myſelf whether, the ſtate of the blood was affected by a putrid diet, the following experiments were performed.

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* Vide Blane on Seamen's Diſeaſes---Medical Tranſactions—and Stark's Works.

EXPERIMENT IV.

July 29th 1792. A dog was put upon a diet of putrid meat and putrid water, and continued to feed thereon till the 27th day of Auguſt following. The meat was beef, and never given to him till it was highly putrid: the drink was clear pump water, rendered putrid by ſuſpending a piece of putrid beef in it, and expoſing it to the action of the ſun.

Auguſt the 12th his eyes were examined, and the pupils of both appeared much contracted. They were watry. At 20 minutes paſt 12 o'clock an ounce of blood was drawn from one of the crural veins. Its colour and ſmell were perfectly natural—it was coagulated at 25 minutes after 12—ſeparation into ſerum and craſſamentum was evident at 30 minutes after 12; the coagulum and ſerum were perfectly natural—the ſerum did not change the colour of a piece of paper, ſtained with a vegetable blue.

Auguſt 19th, he appears weak and very ſick. For theſe few days paſt, he has had a diſreliſh for the putrid meat. His eyes appear ſore and inflamed. I waſhed them with cool pump water. The action of the heart was regular though feeble. Blood was drawn at 4 minutes before 11 o'clock—the colour and ſmell were perfectly natural—it was coagulated at 11—ſeparation into ſerum and craſſamentum was evident, at 6 minutes after 11. The ſerum and coagulum were perfectly natural—The ſerum did not change the colour of the paper ſtained blue.

Auguſt 20th his eyes appear much better—waſhing them with cold water continued. He appears weak.

Auguſt 22d. his eyes appear quite well. Strength ſomewhat recovered. Appetite appears alſo to be encreaſed.

Auguſt 24th blood was drawn at 20 minutes after 5 o'clock. Its colour and ſmell were perfectly natural. It was coagulated at 24 minutes after 5—ſeparation into ſerum and craſſamentum was evident at 31 minutes after 5. The coagulum and ſerum were natural. The ſerum did not change the colour of the paper ſtained with the vegetable blue.

He in general ate and drank a ſufficient quantity; but ate more in the beginning than towards the latter end of the experiment. He was reduced by the diet. The excretions were not evidently affected. The action of the heart was regular as has been mentioned, and reſpiration appeared to be performed in a natural and eaſy manner.

EXPERIMENT V.

A dog was kept faſting from September the 10th 1792, till the 14th of the ſame month.

During his faſting he did not appear uneaſy till the evening of the 13th, when he cried. He continued in this ſtate till the morning of the 14th, when at 15 minutes after 9 o'clock highly putrid broth was given him, he ſwallowed it greedily, belched ſeveral times afterwards, but did not vomit. The action of the heart which before was frequent and feeble, now became ſlower and fuller; he appeared very lively and full of play. At 3 o'clock P. M. he was fed again with putrid broth.

September 15th. At 2 o'clock, he received ſome more of the putrid broth. The action of the heart in every reſpect natural, except that it was little feeble.

September 16th. At three o'clock he was fed again with putrid broth which he took in heartily.

September 17th. At 33 minutes after 9 o'clock blood was drawn from one of the crural veſſels. Its colour and ſmell were natural. It was coagulated at 39 minutes after 9. It began to ſeparate into ſerum and craſſamentum at 50 minutes after 9. The ſerum and coagulum were perfectly natural. The ſerum had no effect in changing the colour of the paper ſtained blue.

EXPERIMENT VI.

A bitch was kept under the ſame circumſtances as the dog laſt mentioned, and the phœnomena were alike in both inſtances.

September 17th. At 12 minutes before 10 o'clock, blood was drawn. Its colour and ſmell were natural. It was coagulated at 6 minutes before 10. It began to ſeparate into ſerum craſſamentum at 3 minutes after 10. The ſerum and coagulum were perfectly natural. The ſerum did not change the colour of the paper ſtained blue.

From theſe experiments, it is evident, that the ſenſible qualities of the blood are not, in the leaſt, affected by a putrid diet. The queſtion then muſt certainly be, *How* *does a putrid diet operate upon the ſyſtem?* I anſwer, by inducing debility from the little nouriſhment it contains; and that from hence the ſolids ſuffer. This I hope to prove by an experiment performed on the dog, the ſubject of the 19th experiment, which will be related under the head of injections of putrid matters into the blood-veſſels.

The proceſs of *digeſtion* is one of the moſt curious operations of nature. It acts wonderfully and powerfully upon our aliment, changes the properties of animal and vegetable matters, reduces them both to a ſubſtance poſſeſſing like properties, and operates upon the hardeſt as well as the moſt fluid ſubſtances.—All this is ſaid to be performed by a fluid we call the gaſtric juice.

If ſuch are the effects of digeſtion on our aliment, is it not reaſonable to ſuppoſe, that the properties of putrid matters taken into the ſtomach may be alſo changed? Let us examine whether putrid food undergoes a change of properties in the ſtomach.

That great Italian Philoſopher, Spallanzani, was I believe, the firſt who made experiments to this purpoſe. He performed them on birds, cats, dogs, and even went ſo far as to ſwallow putrid meat himſelf. He found that the meat, in every inſtance, loſt its putrid ſmell*.

I repeated his experiments on dogs, with the like ſucceſs, and ſhall now relate them.

EXPERIMENT VII.

September 17th 1792. Three ounces of highly putrid beef were given to a bitch. She retained it. Three hours and a half after, the contents of the ſtomach were examined. The meat was found ſurrounded by the gaſtric-fluid; its putrid ſmell was entirely deſtroyed, and its colour appeared more natural than it was before the animal had ſwallowed it.

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* Spallanzani's Diſſertations, Vol. I. p. 284 and ſeq.

EXPERIMENT VIII.

September the 17th. 1792. Highly putrid broth was given to a dog. The ſtomach retained it: and three hours after, its contents were examined, ſome of the broth was remaining together with ſome of the ſolid matters that were mixed with it. The putrid ſmell was found to be abundantly diminiſhed.

I ſhall now proceed to the conſideration of my 3d. head, which has for its object of enquiry, Whether the blood be materially changed and rendered putrid, by the injection of putrid and other ſubſtances into the blood-veſſels?

Many centuries have elapſed ſince phyſicians firſt began to be afraid of the acceſs of the leaſt particle of contagious matter into the blood-veſſels, for, that it excited a fermentation therein, contaminated the whole maſs of blood, and cauſed it to partake of its contagious properties.

If (ſay they) by adding a ſmall quantity of a ferment to a maſs of flour, or other fermentable matter, we are able to bring on a fermentation in it, and convert it into the nature of the ferment, why ſhould not the blood, in like manner, be affected, if by chance or otherwiſe a putrid or other ferment ſhould get into the veſſels and mix with it? Thus they ſuppoſed the ſmall-pox, meaſles, and other eruptive diſeaſes to be produced.

Theſe men always overlooked the vital principle, which exiſts in the one ſubſtance, and not in the other; and here in my opinion the material difference lies; for we know that if the other requiſite circumſtances be duly obſerved, it is only neceſſary to deſtroy life, in order to bring on a putrefaction in the animal. I think, from what I have ſtated, there is ground for believing thatt he blood cannot putrefy in the living animal; but the deductions of reaſon, however juſt and true, the inferences are not to be ſolely relied upon, when the better evidence of ſubſtantial facts—and of facts more immediately in point than the preceding ones, can be had.

To aſcertain beyond a doubt whether the blood could be excited to a putrid fermentation, by injecting putrid matters into the blood-veſſels, became an important and eſſential deſideratum. With this view I made the following Experiments, which were performed *on healthy Dogs*.

The operation of injection was always executed on one or the other of the extremities of the animal. An inciſion was made, and the vein was laid bare, which was for ſome diſtance diſſected free from connection with the adjacent parts. An opening was then made into its cavity, ſufficiently large to admit a curved tube, which was retained in it by means of a ligature paſſed round the veſſel. The tube was made ſo as to ſcrew on the mouth of a common pewter ſyringe.

EXPERIMENT IX.

Was performed July 29th. 1792, on a Bitch, two months old in good health.

About 5 drachms of putrid ſerum were injected into one of her femoral veins. The ſerum was obtained from the blood of an healthy dog; and expoſed in an open phial, for one week; and had a ſmell ſimilar to that of rotten eggs.

Some difficulty attended the introduction of the pipe, by which the matter was injected, owing to its large ſize, and the ſmallneſs of the vein—The vein, when laid bare and touched with any inſtrument, contracted conſiderably, its diameter being thereby much diminiſhed—During the operation, ſhe loſt but a very inconſiderable quantity of blood, yet ſhe ſeemed very weak and languid, probably owing to the pain ſhe endured—Whilſt I was injecting the ſerum into the vein, ſhe had two convulſive paroxyſms, and appeared to be in great pain, which ſhe expreſſed by ſeveral loud cries.—The muſcles of her whole body ſeemed affected with theſe convulſive motions; that they were not owing to the irritation produced by introducing the pipe into the vein is certain, becauſe no ſuch ſymptoms appeared before the injection of the ſerum, though it was introduced twice or thrice.—After the injection was finiſhed, ſhe was placed on the floor, and attempted to walk, but was ſo weak that ſhe fell down. The action of the heart was very frequent, though weak and feeble; reſpiration was anxious and difficult. She ſeemed not at all inclined to move, and was quiet, except that at different intervals, ſhe groaned and ſighed, and afterwards vomited ſome of the food ſhe ate at noon, in an indigeſted ſtate, together with about two ounces of a yellowiſh green coloured fluid.—Continuing in this ſituation, ſhe was frequently affected with convulſive motions of the abdominal muſcles and lower jaw, ſhe became weaker and weaker. At 15 minutes paſt 7 o'clock, about an hour and an half after the experiment was performed, all her muſcles were in a relaxed ſtate, and ſhe appeared motionleſs. At half paſt 7 ſhe was dead.

Diſſection. I did not examine the body till the 30th. at half paſt 8 o'clock; ſo that ſhe was 13 hours dead, when the examination took place.

The abdomen was much diſtended; upon cutting into its cavity, a quantity of putrid air, of a ſmell like that of rotten eggs, ruſhed out. There was an effuſion of a lympid fluid. The inteſtines were diſtended with air, and contained but a ſmall quantity of liqued fœces. The ſtomach contained little elſe, but a large quantity of the before-mentioned flatus, and a fluid of a yellowiſh colour. At its lower orifice it appeared rather preternaturally red. All the other viſcera were in a ſound and natural ſtate. The veins appeared much diſtended; and air, intermixed with with blood, of a dark venous colour, appeared through their coats. When I cut into the veſſels, air came out in bubbles, together with the blood. The blood did not appear to be very firmly coagulated. The heart was much diſtended with blood, particularly the right auricle, which contained ſome air alſo.—In the heart the coagulation was more perfect than it was in the veins. The blood did not ſmell in the leaſt putrid.

EXPERIMENT X.

Was alſo performed July 29th. 1792, on a bitch in good health. About a drachm of pus, diluted with a ſmall quantity of clear pump water, was injected into one of her femoral veins. The pus was obtained on the 27th. inſt. from an abſceſs of the intercoſtal muſcles. This day it poſſeſſes a putrid ſmell.

During the injection of the matter, ſhe had two very violent fits of convulſion, the laſt of which continued for ſome time, and ſhe appeared to be in great pain; reſpiration was quick and irregular; the heart beat frequently, though with ſome force. The pulſations were ſmall and irregular, accompanied with frequent intermiſſions, and to all appearance ſhe was dying. When ſhe was placed in a cool ſituation, the reſpiration became leſs difficult, and the action of the heart more regular. She continued in this ſtate and lay quiet for ſome time, when a mercurial thermometer was applied to the axilla, and the mercury roſe to 100°, as it did when it was applied before ſhe underwent the experiment. After lying about twenty minutes in a cool place, ſhe got up and attempted to walk, but was very weak, and did not go far before ſhe fell down; after which ſhe ſeemed again to recover. At 30 minutes paſt 7 o'clock ſhe was ſeized with twitchings of the muſcles of her abdomen and lower jaw; the weakneſs encreaſed; at length ſhe became motionleſs; and at 15 minutes before 8 o'clock, about an hour and an half after the injection, ſhe was dead.

Diſſection.—The body was not examined till Monday morning the 30th. about 14 hours and an half after ſhe died. The abdomen was conſiderably diſtended, I made an inciſion into it, but found that no air eſcaped—about 2 oz. of a lympid fluid was found effuſed in the cavity. The inteſtines were much diſtended, and when an inciſion was made into them, air and fæces come out. The ſtomach was likewiſe much diſtended with air and half digeſted food. The ſame preternatural redneſs about the lower orifice, which occurred in the former caſe, appeared alſo in this. The other abdominal viſcera appeared perfectly natural and ſound The heart was much diſtended with blood, but no air was found in it or the blood-veſſels. The blood was more firmly coagulated than that mentioned in the laſt Experiment, and it had no unnatural ſmell.

EXPERIMENT XI.

Auguſt 6th. 1792. A drachm of fluid matter, produced by highly putrid beef, was diluted with four drachms of putrid pump water, and injected into one of the femoral veins of an healthy dog.

The matter was injected at half an hour after 11 o'clock. During the injection he was much convulſed, and appeared to be in great pain. The pupils of the eyes were ſomewhat dilated. The beating of the heart was now very frequent, though feeble, as it was during the injection of the matter. At 15 minutes before 12 o'clock the breathing became very frequent, accompanied with great difficulty and ſighing. At this time he vomited a quantity of half digeſted food, and appeared to be greatly relieved by it; at 12 o'clock he vomited again, which relieved him ſo that he roſe, but in one or two minutes laid down again. At 4 minutes paſt 12 he had an evacuation of fæces—at 7 minutes after 12 he attempted to lay down, but fell, and laid in an unnatural poſture—he appeared to be very weak—his breathing became more difficult and laborious—his eyes appeared watry and ſunk in the orbits. At 10 minutes paſt 12 he had another evacuation of fæces. At 15 minutes after 12 he was raiſed up on his feet—he ſtood, though with difficulty, and his left hind leg became paralytic *. In a ſhort time all his hind parts appeared to be more or leſs affected in this manner. At 40 minutes after 12 he became very reſtleſs, turning and twiſting his body every way. At 45 minutes after 12 he was again raiſed upon his feet, but could not ſtand, for all his muſcles appeared greatly relaxed. At 10 o'clock the pupils of his eyes were much dilated, and his ſight ſeemed to be greatly diminiſhed—he had an evacuation of urine. He began to groan, and the abdominal muſcles were convulſed. The convulſions, after continuing ſome time in thoſe parts, extended to the muſcles of his head and neck. Reſpiration, and the action of the heart ceaſing, he died at 12 minutes paſt 10 o'clock.

Diſſection. The body was examined immediately after death, and nothing unuſual was obſerved. The blood was quite natural in every reſpect.

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* The inciſion in this Experiment was made on the *right* thigh.

EXPERIMENT XII.

Auguſt 10th, 1792. Twelve grains of putrid blood, diluted with a drachm of clear pump water, were injected into one of the femoral veins of an healthy bitch, at thirty-five minutes paſt 10 o'clock. During the injection ſhe ſeemed uneaſy, and had an evacuation of urine. The action of the heart became much ſlower, and very feeble. Reſpiration was ſomewhat difficult. She was then placed on the floor, and continued ſtanding —12 minutes after ſhe had a natural motion from the rectum, and then laid down. At 11 o'clock her eyes were examined, and they did not appear any way preternatural. She ſeemed dull, heavy, and much inclined to ſleep. At 12 o'clock I perceived that ſhe had had another evacuation of urine. The eyes were now examined again, and no alteration appeared to have taken place in them. She laid quiet till half paſt 1 o'clock, when violent efforts to vomit came on, and ſhe brought up a quantity of the food ſhe had eaten previouſly to the performing of the experiment. The heart beat very frequent and feeble—reſpiration was not greatly affected, but ſhe continued dull and heavy. At 2 o'clock meat and drink were offered her—ſhe would not even bear the ſmell of meat, but roſe and drank ſome water. She laid down. At half paſt 2 o'clock ſhe got up again, walked about, and evacuated urine; then laid down again, and was dull and heavy as before. At 5 minutes before 5 o'clock ſhe had another evacuation of urine. At 3 minutes before 5 ſhe had a very copious evacuation of extremely fluid fæces, of the colour of coffee-grounds, and of a very putrid ſmell. She now appeared weaker than ſhe was before the evacuation, though not quite ſo dull. At 6 o'clock ſhe evacuated urine again. I left her at 25 minutes after 6, when circumſtances did not appear much altered. At 8 o'clock I ſaw her again, meat and drink were offered her—ſhe drank, but would not eat. The heart beat very frequent and feeble—in other reſpects as before.

Auguſt 11th. This morning, at 8 o'clock, I found her dead. A diſagreeable odour aroſe from her body. The abdomen appeared ſomewhat diſtended. I perceived ſhe had had another looſe evacuation of the deſcription above mentioned, though not ſo copious as the former one. There was a great deal of ſaliva about the mouth, and the tongue protruded through the teeth. On

Diſſection, the following appearances were obſerved. Upon cutting into the cavity of the abdomen, no air eſcaped, but I experienced a very diſagreeable fœtid ſmell. The ſuperior parts of the inteſtines appeared in ſeveral places of a dark green colour, ſpotted with ſmall white ſpecks, while the lower portions appeared natural. When an inciſion was made into the inteſtines, a quantity of putrid air ruſhed out, together with liquid dark green coloured fæces. The ſtomach appeared to be rather ſmall—I made an inciſion into it when ſome of the like liquor flowed out. The liver was in many places of a preternatural colour, and adhered to almoſt all the other abdominal viſcera, particularly to the ſtomach and right kidney. The gall-bladder was much diſtended with a light green coloured bile; the lungs were collapſed; the right lobes were of a blackiſh colour, intermixed with red; the left lobes appeared natural. The red appearance at the lower orifice of the ſtomach, mentioned in the other caſes, was not apparent in this. The veins and heart were much diſtended with blood, which was not very firmly coagulated, but its ſmell was perfectly natural.

EXPERIMENT XIII.

Auguſt 14th. 1792. At 40 minutes after 10 o'clock ſix grains of putrid blood, mixed with a drachm and an half of clear pump water, were injected into one of the femoral veins of a bitch. During the injection ſhe appeared very uneaſy—the action of the heart was ſlow and feeble—reſpiration ſlow, and performed with difficulty. She was placed on the floor, appeared dull, and laid down. At 10 minutes before 11 o'clock her abdominal muſcles were violently convulſed. At 7 minutes before 11 ſhe was ſeized with violent retchings and efforts to vomit, but did not evacuate. At 15 minutes before 3 o'clock meat and drink were offered her—ſhe ate, but would not drink. At 23 minutes before 7 ſhe had a copious evacuation of urine. At 20 minutes before 7 ſhe ate and drank. I left her at 15 minutes before 7, when ſhe ſeemed eaſy. I ſaw her again at 8 o'clock and no alteration was eveyident.

Auguſt 15th. This morning, at 8 o'clock, I ſaw her—ſhe appeared perfectly eaſy—action of the heart nearly natural, though rather frequent. She was now fed, and ate as before. I perceived that ſhe had had a natural evacuation of fæces. At 21 minutes after 5 blood was drawn—its colour and ſmell were natural—it was coagulated at 24 minutes after 5—the ſeparation into ſerum and

craſſamentum

craſſamentum was evident at 30 minutes after 5—a piece of the paper ſtained blue was dipped into the ſerum, and no change of colour took place—the ſerum and coagulum were quite natural. I ſaw her again at 8 o'clock. She appeared as ſhe did when in health.

Auguſt 16th. She was perfectly well.

EXPERIMENT XIV.

Auguſt 17th. 1792. At 15 minutes after 12 o'clock twelve grains of putrid blood, mixed with a drachm and an half of clear pump water, were injected into one of the humeral veins of the bitch laſt mentioned. During the injection, ſhe cried violently, and appeared to be in great pain. Reſpiration became very frequent—action of the heart frequent and feeble. The eyes were examined, and the pupils were found to be much contracted. She was placed on the floor, walked a few ſteps, leaned againſt the wall, in a ſtanding poſture, and appeared very ſick. At 26 minutes after 12 ſhe had an evacuation of fæces, which was rather looſe. At half paſt 12 ſhe laid down, appeared dull, and much inclined to ſleep. At half paſt 2 the abdominal muſcles were affected with convulſive contractions—they did not continue long. The eyes were again examined, and the pupils appeared natural. At 3 minutes before 5 ſhe had a copious evacuation of urine. At 10 minutes after 6 I left her eating, and to appearance eaſy, though dull and languid. I ſaw her again at 8 o'clock, when I perceived ſhe had had another looſe evacuation of fæces, which was copious. Pulſation of the heart frequent and feeble—ſhe appeared eaſy.

Auguſt

Auguſt 18th. Blood was drawn at 5 minutes before 6 o'clock P. M.—its colour and ſmell were natural —it was coagulated at 6—at 5 five minutes after 6 it ſeparated into ſerum and craſſamentum—coagulum and ſerum were quite natural—Serum did not change the paper ſtained blue. At 8 ſhe appeared perfectly well.

Auguſt 19th. She was perfectly well. The blood, which was drawn yeſterday, had a natural odour, when it was examined this morning at 11 o'clock.

EXPERIMENT XV.

Auguſt 20th. 1792. At 4 minutes before 11 o'clock, half a drachm of putrid blood and a drachm of clear pump-water, were injected into one of the humoral veins of the laſt mentioned bitch during the injection, ſhe was very uneaſy and gave ſeveral loud ſhrieks. The heart beat very frequent and feeble. Reſpiration became very difficult. She was placed on the floor, and immmediately after had a copious evacuation from the ſtomach. She appeared very weak, and leaned againſt the wall. At 7 minutes after 11, ſhe had retchings and violent efforts to vomit, b ut no evacuation enſued. At 32 minutes after 11, ſhe began to groan and ſigh. At five minutes after 12, ſhe had efforts to vomit, but nothing was thrown up. I left her at 15 minutes before 1; ſhe drank, and appeared eaſy, though ſhe was very weak. I ſaw her again at 2 o'clock, the heart beat very frequent and feeble. I perceived ſhe had had an evacuation of fæces, while I was abſent—it was rather looſe. Meat and drink were offered her; ſhe drank, but did not eat. She appeared very ſick.

I ſaw

I ſaw her again at 8'clock, when ſhe appeared juſt as ſhe was at 2 o'clock.

Auguſt 21ſt. Blood was drawn at 2 minutes before 5 o'clock P. M. its colour and ſmell were natural—it was coagulated at 3 minutes after 5—ſeparation into ſerum and craſſamentum was evident at 9 minutes after 5—the coagulum and ſerum were perfectly natural—the ſerum did not change the color of the paper ſtained blue. At 8 o'clock ſhe appeared very well.

Auguſt 22d. Serum of the blood drawn yeſterday, did not change the colour of the paper ſtained blue.

EXPERIMENT XVI.

Auguſt 22d. 1792. At 11 o'clock, a drachm of putrid blood mixed with half a drachm of clear pump water, was injected into one of the crural veins of the bitch laſt mentioned. During the injection, the action of the heart became very frequent and feeble. Reſpiration very laborious. She vomited. After this ſhe was placed on the floor, and appeared very weak, yet ſtood for ſome minutes, and then fell down gently. At 8 minutes after 11, ſhe had an evacuation of urine and of fæces. At 14 minutes after 11, the eyes were examined; the pupils were found to be very much contracted, and her ſight much diminiſhed. At 16 minutes after 11, ſhe had another evacuation of urine. At 19 minutes after 11, ſhe began to cry violently, and appeared to be in very great pain. The reſpiration became very laborious, and the action of the heart encreaſed in frequency and feebleneſs. At 23 minutes after 11, ſhe became convulſed. At 25 minutes after 11, a finger was drawn over the eye, and no contraction of the eye-lids took place. At 28 minutes after 11, ſhe was dead.

Diſſection. The examination of the body took place at 8 minutes after 12 o'clock. All the viſcera were found in a ſound and natural ſtate, except the lungs—there a bloody effuſion was diſcovered, particularly in the inferior parts of the left lobes. At 20 minutes after 12, blood was obtained by opening one of the large veins; the colour and ſmell were natural. When I left the blood, it was not ſo perfectly coagulated, as in the other inſtances, owing to its having been by accident much agitated. I returned in the afternoon, and found the coagulation complete—the coagulum and ſerum were natural—the ſerum did not change a piece of paper ſtained blue.

EXPERIMENT XVII.

Auguſt 14th. 1792. At 16 minutes after 3 o'clock, ſix grains of putrid blood, diluted with a drachm of clear pump water, were injected into one of the femoral veins of an healthy dog. During the injection, he was very uneaſy and gave ſeveral loud ſhrieks. The action of the heart became ſo feeble, that it could ſcarcely be felt, and was alſo frequent. He was placed on the floor, when he lay down. In a ſhort time he roſe up, ſtood ſometime, and appeared very weak. Reſpiration became laborious, and he lay down again. At 15 minutes before 4 o'clock, he was ſeized with twitchings about the botttom of, and acroſs the thorax, in a great degree reſembling an hiccup.

At 4 minutes after 4, meat and drink were offered him; he would not drink, and ſeemed as if deſirous to eat; but when he approached near the meat, he drew his head from it, as if the ſmell of it were offenſive to him, though the meat was freſh killed this morning. At 2 minutes before 5, he had an evacuation of urine, and rather a looſe evacuation from the rectum. At 4 minutes after 5 he had a copious evacuation from the ſtomach, when the action of the heart became perceptible to the touch. At 20 minutes after 5, he had violent efforts to vomit, and brought up a quantity of greeniſh coloured fluid. At 34 minutes after 5, they recurred again, with the like effect. They attacked him a third time at 3 minutes before 6; the conſequence was the ſame as in both the former inſtances. I left him at 15 minutes before 7 o'clock, when he was eating and drinking. He ſeemed eaſy.

I ſaw him again at 8 o'clock, when he appeared heavy. I perceived that ſince I had left him, he had had a ſparing evacuation of fæces, rather looſe.

Auguſt 15th. This morning at 8 o'clock, he appeared dull, heavy and weak. The action of the heart was frequent and feeble. He was now fed. At 2 o'clock, I ſaw him again; he was as deſcribed in the morning.

At 5 o'clock, blood was drawn—the colour and ſmell were natural—it coagulated at 4 minutes after 5—ſeparation into ſerum and craſſamentum took place, at 12 minutes after 5—the colour of the paper ſtained blue was not changed by the ſerum—The coagulum and ſerum were perfectly natural—I ſaw him again at 8 o'clock, and he appeared perfectly well.

Auguſt 16th. He was perfectly well to-day.

EXPERIMENT XVIII.

Auguſt 17th. 1792. At 15 minutes after 4 o'clock, ten grains of putrid blood, mixed with a drachm of clear pump water, were injected into one of the femoral veins of the dog laſt mentioned. During the injection, he appeared to be in much pain, and had an evacuation of urine; the action of the heart became frequent and feeble, but reſpiration was not much altered. At half paſt 4, the abdominal muſcles were convulſed; the convulſions did not laſt any length of time. At 20 minutes before 5, he was ſeized with tremors over his whole body; they laſted about two minutes, and then went off. At 16 minutes before 5, he had an evacuation of urine. At 15 minutes after 6, I left him eating, when he appeared pretty eaſy. I ſaw him again at 8 o'clock, and perceived he had had a motion ſince I left him. The action of the heart was frequent and feeble. While I was with him he had an evacuation of urine, and appeared eaſy.

Auguſt 18th. Blood was drawn at 10 minutes after 6 o'clock this morning—its colour and ſmell were natural—it was coagulated at 14 minutes after 6—ſeparation into ſerum and craſſamentum began to take place at 23 minutes after 6. The coagulum and ſerum were natural. The ſerum did not change the colour of the paper ſtained blue. At 8 he appeared to be perfectly well.

Auguſt 19th. He was perfectly well to-day. The blood, that was drawn yeſterday, was examined this morning, and poſſeſſed no unnatural ſmell.

EXPERIMENT XIX.

Auguſt 27th. 1792. At 40 minutes before 10 o'clock, a drachm of putrid blood, mixed with half a drachm of clear pump-water was injected into one of the femoral veins of the dog, the ſubject of the 4th. Experiment. During the injection, he was very uneaſy, and evacuated urine. The action of the heart became very frequent and feeble. He was placed on the floor, and immediately after vomited. At 3 minutes before 10, he had an evacuation of natural fæces. At 2 minutes before 10, he vomited again. At 10 his breathing became very laborious, and he had a looſe and ſmall evacuation of natural coloured fæces. At 3 minutes after 10, he lay down. The heart beat ſo feeble, that it could ſcarcely be felt. At 10 minutes after 10, he roſe, vomited again, and then lay down. At 15 minutes after 10, the eyes were examined and no alteration in them was apparent. At 21 minutes after 10, he roſe again, walked about the room, ſtood for ſometime, and then again lay down. The action of the heart became more evident. At 11 o'clock, he groaned very much. At 6 minutes after 11 the action of the heart became more frequent and feeble. The eyes were now again examined, but preſented no unnatural appearance. At 20 minutes after 11, he roſe and walked a few ſteps, had a ſparing evacuation of chocolate-coloured, liquid fæces, then fell down, and appeared as if ſtimulated to evacuate again; he roſe and had an evacuation of urine, and again fell down. At 18 minutes before 12, his abdominal muſcles became convulſed. At 4 minutes before 12, he vomited again. At 28 minutes after 12, he roſe, walked a few ſteps, had an evacuation of very thin chocolate-coloured fæces, and appeared to be very weak; after this he walked a few ſteps again, and then lay down. I left him at 10 minutes before 1 o'clock, when he appeared eaſy, though very weak and ſick. When I returned, at 12 minutes before 3, I perceived, that during my abſence, he had had two or three evacuations of urine. The heart beat frequently and feebly. At 10 minutes before 3, he had an evacuation of urine, and afterwards vomited. At 8 minutes before three, he had an evacuation of very liquid fæces, intermixed with mucus. At 5 minutes before 3, he had retchings and efforts to vomit, but did not evacuate. At 16 minutes before three, he roſe, and had another evacuation of fæces ſimilar to the one laſt mentioned. At 10 minutes after 4, he had an evacuation from the rectum, of mucus intermixed with blood. At 25 minutes after 4, putrid meat and putrid water were offered him, he drank plentifully, but did not eat. I left him at half paſt 4, and ſaw him again at half paſt 7, when I perceived he had had a very copious evacuation of urine, but had not eaten. The heart beat frequently, though not ſo feebly as before. Reſpiration was pretty free. He ſeemed eaſy, and appeared much better than when I left him the laſt time.

August 28th. When I ſaw him this morning at 8 o'clock, marks of an evacuation of fæces and urine were evident. I perceived he had eaten nothing. Putrid water was given him and he drank of it. The heart beat frequently and feebly. He appeared very weak; and the wound put on a bad appearance. At 2 o'clock I ſaw him again, when I perceived that he had had two or three evacuations of urine, but that he had not eaten. I now offered him ſome freſh meat, he held it in his mouth, but did not ſwallow any of it, and let it drop. Putrid water was again offered to him and he drank it. Reſpiration did not appear to be much affected. The action of the heart was frequent, and ſo feeble as ſcarcely to be felt. In my preſence he had an evacuation of urine; I caught ſome of it in an earthen veſſel, dipped a piece of the blue-coloured paper in it, but no change of the colour was evident. The wound appeared in a gangrenous ſtate. He ſeemed very weak, and it was with difficulty that he ſtood. At 8 minutes before 5, blood was drawn; the colour and ſmell were natural. It coagulated at 4 minutes before 5; ſeparation into ſerum and craſſamentum was evident, at 7 minutes after 5; the ſerum and craſſmentum were perfectly natural; the ſerum did not change the colour of the paper ſtained blue. The wound appeared much worſe. In every other caſe, yet mentioned, it invariably put on a good appearancee, and healed readily. After bleeding he appeared exceedingly weak. At 35 minutes after 5, I left him very uneaſy. I ſaw him again at 8 o'clock, and he appeared nearly in the ſame condition as when I left him laſt.

Auguſt 29th. This morning at 8 o'clock, I found him dead. The blood drawn yeſterday, was now examined. No unnatural ſmell was evident. The ſerum did not change the colour of the paper ſtained blue. The coagulum was ſo firm, that when thrown out of the tumbler on the floor, it did not break. A very diſagreeable and fœtid odour aroſe from the body.

Diſſection. The body was examined at half paſt 9 o'clock, when I cut through the ſkin covering the lower ribs, the fleſh below appeared gangrenous. The liver in ſome places was of rather a lighter colour than natural. The inferior part of the ſmall inteſtines appeared inflamed. The ſtomach was perfectly natural, both internally and externally; it contained mucus and a whitiſh fluid. A bloody effuſion was found in the lungs, particularly in the right lobes. The bladder was quite natural. The heart and veins were much diſtended with blood, which was firmly coagulated in both; the colour and ſmell of it were perfectly natural.

As yeaſt is well known to be a powerful ferment, and the volatile alcali a great chemical agent, and a diſſolver of the blood when out of the body, the following Experiments were made, to aſcertain what effect they would have on the blood, when injected into the blood-veſſels.

EXPERIMENT XX.

Auguſt 14th. 1792. At 20 minutes after 11 o'olock three drachms of ſtock-yeaſt were injected into one of the femoral veins of a dog. During the injection he appeared ſomewhat uneaſy. Reſpiration became very difficult and laborious; the action of the heart irregular, intermitting, and ſomewhat encreaſed in force. He was placed on the floor but was not able to ſtand. At 30 minutes after 11, he had an evacuation of urine; at 33 minutes after 11, he had an evacuation of natural fæces. The eyes being examined, did not appear evidently altered. The muſcles of his whole body now ſeemed to be in a relaxed ſtate. At 40 minutes after 11, his eye-ſight appeared to be much diminiſhed; at 5 minutes before 12, he was dead.

Diſſection. The body was examined immediately after death. Nothing preternatural was obſerved, except a bloody effuſion in the lungs. The blood was in every reſpect natural.

EXPERIMENT XXI.

Auguſt 7th. 1792. At 20 minutes before 11 o'clock, 15 grains of mild volatile alcali diſſolved in two drachms of clear pump water, were injected into one of the femoral veins of an healthy bitch. During the injection, ſhe gave three or four loud cries, and ſeemed to be in great pain. She was placed on the floor, walked three or four ſteps, and then lay down. The action of the heart was encreaſed in frequency, and reſpiration was performed with difficulty. In other reſpects ſhe ſeemed eaſy and quiet. At 11 o'clock, meat and drink were offered to her; ſhe would not take of either. Reſpiration now ſeemed to be performed with no great difficulty. The action of the heart was as before mentioned. At 35 minutes paſt 4, the eyes were examined, and they appeared no ways altered. At 42 minutes paſt 11, ſhe began to be reſtleſs. The action of the heart ſeemed to have recovered ſome degree of quickneſs; the frequency of it ſtill continued. Reſpiration appeared pretty eaſy, though ſhort. At 40 minutes paſt 12, her whole body was ſeized with a tremulous motion, when a thermometer was applied to the axilla, and the temperature was as when in health. Theſe tremors came on in paroxyſms, each of which laſted but a little time, and they recurred frequently. The heart, during a paroxyſm of trembling, beat frequently and feebly. The tremors encreaſed in violence, and in frequency of recurrence. At 14 minutes paſt 1, ſhe was affected with ſeveral violent convulſive contractions acroſs her abdomen. At half paſt 1, the eyes were again examined, and the pupils were obſerved alternately to dilate and contract frequently and conſiderably. At 40 minutes after 1, ſhe roſe, looked about; meat and drink were again offered her, but ſhe refuſed both, and lay down again. At 10 minutes before 2, the tremors attacked her again, her breathing became more difficult and laborious, the tremors continued more or leſs violent for 5 minutes, then went off, and ſhe appeared eaſy. At 15 minutes after 2, the tremors and laborious breathing came on again; they were of ſhort duration; when they went off ſhe appeared eaſy, and continued ſo till 5 minutes after 4, when they occurred again. At 10 minutes after 4, ſhe gave three or four loud cries, as if affected with much pain, then was eaſy again, and remained ſo till 40 minutes after 5, when I left her. At 8 o'clock I ſaw her again; ſhe ſeemed perfectly eaſy, the action of the heart was frequent and feeble.

Auguſt 8th. At 8 o'clock this morning ſhe appeared eaſy, but was very weak; and her heart beat frequently and feebly. She now ate and drank; at 2 o'clock ſhe appeared as in the morning: I alſo perceived that ſhe had had a natural evacuation from the rectum ſince I ſaw her in the morning, which was the firſt that occurred ſince the operation was performed. At 7 o'clock I ſaw her again; no alteration was evident. I found that ſhe had had a natural evacuation of fæces this afternoon; but I did not perceive any marks of a diſcharge of urine.

Auguſt 9th. She ſeemed quite well, but drank more than dogs uſually do in health.

EXPERIMENT XXII.

Auguſt 10th. 1792. At 40 minutes after 11 o'clock, 25 grains of mild volatile alcali, diſſolved in two drachms of clear pump-water were injected into the bitch laſt mentioned. During the injection, ſhe was violently convulſed, and gave ſeveral loud cries; ſhe alſo had an evacuation of urine and vomited. The heart beat very frequently, and breathing was laborious. She was placed on the floor, ran about the room, and then lay down. Her aſpect was very wild. She did not lay long before ſhe got up again; but in a few minutes lay down. At half paſt one ſhe roſe, and immediately afterwards lay down again. The breathing was very frequent. At 2, meat and drink were offered her, but ſhe did not take of either. I left her at 25 minutes after 6, ſhe continuing to be in the ſame condition. At 8 o'clock I ſaw her again; meat and drink were then offered her, ſhe drank, but would not eat. Her heart beat very frequently, though weak; ſhe ſeemed in other reſpects as ſhe was before.

Auguſt 11th. This morning at 8 o'clock ſhe appeared to be eaſy, was quiet, and ate and drank. Her heart beat frequently and feebly. I ſaw her again at 8 in the evening; ſhe was in the ſame condition as in the morning. At 26 minutes after 8 blood was drawn—its colour and ſmell were quite natural—it coagulated at 32 minutes after 8 o'clock—ſeparation into ſerum and craſſamentum was evident at 38 minutes after 8 o'clock—the coagulum and ſerum were every way natural—the ſerum did not change the colour of the paper ſtained blue.

Auguſt 12th. To-day ſhe appeared perfectly well.

EXPERIMENT XXIII.

Auguſt 13th. 1792. At 15 minutes before 11 o'clock 45 grains of mild volatile alcali diſſolved in two drachms and an half of clear pump water, were injected into the ſame bitch. During the injection ſhe appeared to be in great pain, and gave ſeveral loud cries. She was placed on the floor, ran about the room, then lay down, and was very reſtleſs. The action of the heart was extremely frequent, and her aſpect was wild. After lying a few minutes ſhe became eaſy. At 35 minutes after 2 meat and drink were offered her; ſhe ate, but would not drink. At 30 minutes after 4 o'clock blood was drawn—its colour and ſmell were natural—it coagulated at 35 minutes after 4—ſeparation into ſerum and craſſamentum was evident at 39 minutes after 4—the craſſamentum and ſerum were perfectly natural—the ſerum did not change the colour of the paper ſtained blue. At 39 minutes after 4 more blood was drawn from the ſame vein—its colour and ſmell were natural: As ſoon as it was drawn, I added 45 grains of mild volatile alcali diſſolved in ʒij of clear pump water to it, when the colour became a very deep brown, nearly black. At 8 minutes before 5 o'clock it appeared to have a tendency to coagulate. At 10 minutes before 6 it was of the conſiſtence of mucus. At 10 minutes after 6 ſhe ſeemed perfectly eaſy, and was eating meat. At 8 o'clock ſhe appeared in the ſame eaſy ſituation.

Auguſt 14th. At 8 o'clock this morning ſhe appeared perfectly eaſy. The blood laſt mentioned was examined, and its conſiſtence was much as before.

From the above Experiments the following inferences reſult:

1ſt. That, contrary to the generally received opinion, the blood is neither rendered alcaline, acrid or putrid by ſtarvation. This I think is evinced by theſe circumſtances—that the blood went through its ſpontaneous changes in the regular and uſual manner—that the ſerum had no effect in changing the colour of my teſt—and that no ſenſe of acrimony was perceptible to the taſte.

2dly. That a Putrid Diet does not operate upon the blood ſo as to change its ſenſible properties. This is a material part of the enquiry, ſince phyſicians, from Hippocrates down to the preſent day, have ſuppoſed various and wonderful changes to be produced in the animal œconomy by ſuch food. From the above Experiments, it appears very clear, that a putrid diet had no effect whateever in changing the qualities of the blood, although the animals were ſtrictly confined to it. We are alſo led to conclude from them, that a putrid diet does not change the ſenſible qualities of the excretions; ſince, as I have related, the urine did not alter the colour of the vegetable blue. Here it may be objected to me, that I ought merely to infer from theſe Experiments, that the blood of dogs only is not affected by this treatment—I anſwer, that the inference may, probably, be extended to the whole animal creation; for, we cannot but ſuppoſe that nature is, in this reſpect, as beneficent to the one ſpecies as ſhe is to the

other;

other; and to ſubſtantiate this opinion, I avail myſelf of the reſult of the Experiments of the celebrated Spallanzani as above mentioned, who not only operated upon the brute creation, but went ſo far as to riſk his own life for the benefit of ſcience; and found, contrary to the received opinion, that vitiated food, previous to its leaving the ſtomach, is converted into a matter capable of furniſhniſhing good blood. "Men, ſuch as the inhabitants about the mouth of the Orange river, in Africa, live always on animal food, ſuch as whales, ſeals, limpets, and what fiſh they can catch; that many times their food has entered into a great degree of putrefaction, and there is no vegetable food whatever employed at the ſame time; probably moſt of them never taſted any vegetable ſubſtance in their lives, excepting aromatics for ſeaſoning; yet they are perfectly healthy and free from all putrefaction in their fluids or ſolids, though they are not very careful of avoiding it in the exterior parts of the body. We ſee likewiſe maggots live in and upon putrid maſſes, while they themſelves, and all their fluids, are perfectly ſweet and free from all appearance of putrefaction*." There appears to be an intimate connection between the fluid and ſolid parts of our bodies, but how far this connection between them ſubſiſts, I will not undertake to aſſert. Perhaps I might, in truth, have ſaid that a putrid diet acts upon the ſolids of our machine, by its not containing alimentary matter in a proper quantity, and therefore not affording a ſufficiency of blood to ſupport the animal. We know that the ſolids derive their nouriſhment from the blood; and, in my opinion, it is ſufficiently evidenced, from the mortification of the muſcular parts which enſued upon making an inciſion into the thigh, as has been already ſtated—That a putrid diet does operate upon the ſolids.

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* Fordyce on Digeſtion, p. 155.

3dly. That although the blood, or other animal, matter may be excited into a putrid fermentation out of the body, by the addition of a putrid ferment, yet that ſuch a proceſs cannot be excited in the living body. We have found that many grains of putrid matter exiſted in the blood-veſſels for ſome days without changing the blood. Is it probable that a fermentation can be induced in the blood when the ferment enters the ſyſtem by abſorption, and no ſuch proceſs take place by introducing a putrid ferment immediately into the blood-veſſels?

4thly. That by the introduction of putrid matter into the blood-veſſels, very violent ſymptoms were produced, although the quantity of the matter was ſmall, and of the ſame nature as the blood, except that it had become putrid*. Is it probable, then, that the animal could exiſt with the whole maſs of blood in a ſtate of putrefaction, when ſuch violent ſymptoms were the conſequence of the introduction of ſo ſmall a quantity of putrid matter as was injected in the foregoing Experiments?

And, 5thly, That though the volatile alcali may operate upon the blood in a wonderful manner out of the body, it does not evidently affect the blood when injected into the blood-veſſels.

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* My reaſon for making uſe of putrid ſerum and putrid blood, is, that they are the ſame ſubſtance, only altered by putrefaction. We know, from Experiments of Transfuſion, that blood may be paſſed from the veſſels of one animal, into thoſe of another, without any evident injury. From this circumſtance we are led to infer, that it, in theſe inſtances, acted as putrid matter.

From theſe experiments it is alſo evident, that cathartic and emetic medicines, when injected into the blood-veſſels, cannot operate in a ſpecific manner; for in almoſt every inſtance evacuations were the conſequence of the experiments when neither cathartics nor emetics were injected. Probably any matter capable of producing a ſufficient irritation, will produce thoſe effects, when thus thrown into the animal.

I attribute the preſence of air in the veſſels and heart, which occurred in one of the experiments, to the imperfect ſtate of the ſyringe; for it conſiſted of one whole piece, when the two firſt experiments were performed.

Before I leave this part of the enquiry, I beg leave to ſtate the following queſtions, viz.

Is there a power in the blood-veſſels, or in the blood, capable of aſſimilating to the blood matters which are injected into the veſſels? Do not the following circumſtances make it probable? I could not by a minute examination diſtinguiſh between the blood and the matter injected. The ſerum of the blood, where the volatile alkali was injected did not poſſeſs the odour of that ſalt, neither did it change the colour of the paper ſtained with a vegetable blue. Does not the doctrine of ſecretion, which is now gaining ground, favour ſuch an idea? I own that there are certain exceptions to this opinion.

Do not the loſs of viſion, the dilated and contracted pupil, together with the convulſions which happened, denote the nervous ſyſtem to be affected by the contents of the blood-veſſels?

I ſhall now proceed to enquire, whether there is any reaſon to believe that the blood becomes putrid in any diſeaſe?

This is a queſtion of conſiderable importance. Extenſive experience is neceſſary to decide it; and as my own has been very limited with regard to diſeaſes ſuppoſed to be putrid, I muſt beg leave to refer the unprejudiced reader to books, which treat at large of them. Perhaps upon thorough conſideration, he will believe the matter to be at leaſt doubtful.

All I can do, will be to ſtate ſome objections to the inferences that have been drawn from the ſymptoms and circumſtances attending theſe diſeaſes.

A ſymptom on which great ſtreſs has been laid, is, the appearance of Petechiæ, Vibices, or effuſions of blood, which takes place in the laſt ſtage of Typhus, Yellow Fever, Scurvy &c. Theſe appearances are generally ſuppoſed to be indicative of a diſſolved ſtate of the blood, in thoſe diſeaſes, though in my opinion without a ſufficient reaſon. It is conceded by all that the ſyſtem in thoſe diſeaſes is very much debilitated, and of conſequence the ſyſtem of blood-veſſels, and theſe particularly at their ultimate terminations. The effect of this debility is a relaxation of the ſolids; the fibres of the blood-veſſels will not now be in as cloſe contact as they were in health, and the mouths of the exhalents will not be as narrowly contracted. This particular ſtate of the veſſels at their terminations, will allow the blood to tranſude, and be effuſed in the cellular membrane; or the exhalents, which in health pour out a lympid fluid, may now be capable of forwarding the red blood itſelf. Analogy ſupports the opinion. Do not the veſſels of the eye in ophthalmia, through which a colourleſs lymph is circulated in health, receive in this diſeaſed ſtate, red globules? Do we not find in dropſy, that the veſſels allow of a greater exhalation than when in health? If in proportion as we become debilitated, we approach nearer to a ſtate of death, Does not the tranſudation of bile through the coats of the gall-bladder, in dead ſubjects, give additional ſtrength to the idea? Again, is it not confirmed by the circumſtance, that theſe appearances take place in the laſt ſtage of thoſe diſeaſes when the debility is greateſt?

If Petechiæ were true ſymptoms of a diſſolved and putrid ſtate of the blood, we ſhould certainly find that the bodies of thoſe who die, marked with ſuch ſymptoms, would certainly putrefy much ſooner than the bodies of others who die without any ſuch marks. The contrary of this is ſaid to happen: And anatomiſts aſſert "that the bodies of thoſe who die, of what are called putrid fevers, are longer before they undergo real putrefaction than of thoſe who die of other diſeaſes, or who die in perfect health by violence." *

The diſſections of the celebrated Lind and Rouppe, ſeem to prove that the blood in thoſe diſeaſes is not putrid. The former found it in the yellow-fever to be grumous and viſcid, and covered with a yellow gluten, impenetrable to the finger unleſs cut by the nail †

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* Moore's Medical Sketches.

† Lind's firſt paper on Fevers and Infection, p. 13, & ſeq.

The ſame gentleman relates; "that in ſcurvy, red coagulated blood is found extravaſated in almoſt all parts of the body, not only into the tela celluloſa, but into the bellies of the muſcles, particularly of the legs and thighs, which often become quite ſtuffed, and even diſtorted, with large grumous maſſes *."

After ſtating that, "Some authors from a ſuppoſition of the great diſſolution of the blood in petechial fevers, and from another ſuppoſition, that bliſters encreaſed the diſſolution of that fluid ſtill more, have forbid their application in ſuch fevers." He adds, "But the experience of the moſt eminent practitioners does not ſupport thoſe theoretical opinions †."

Rouppe, a phyſician who had great experience in the ſcurvy, found that the blood was always more or leſs coagulated in thoſe inſtances, in which he had an opportunity of viewing it. He obſerves, "If we conſider, we ſhall find that the blood of thoſe who have a continual fever, or an inflammation, after they have loſt a good deal by bleeding, is ſo diſſolved, that no one can find blood of a thinner conſiſtency even in the laſt ſtage of the ſcurvy. Who (he juſtly interrogates) has even pretended to ſay theſe diſeaſes were owing to a diſſolved or putrid ſtate of the blood ‡?"

We may further obſerve, that a diſſolved ſtate of the blood is by no means a proof of its having become putrid, it merely indicates a leſſened diſpoſition to coagulate; and

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* Lind's 2d paper, p. 100.

† Lind's 2d paper, p. 87.

‡ Rouppe on the Diſeaſes incidental to Seamen, p. 200.

do we not find the blood in ſeveral other diſeaſes beſides the ſcurvy &c. to be in a more fluid ſtate than it is in health? Certainly we do.

We are informed by Dr. Lind, that the blood of ſcorbutic perſons did not impart the leaſt ſenſe of acrimony to the tongue, any more than the white of an egg; and that the blood of ſcorbutics does not become putrid ſooner than other blood, which it certainly ought, cæteris paribus, if it had already begun to putrefy in the body. Nay we are moreover informed by this celebrated writer, that the ſerum of the blood of ſuch patients, is not ſceptic but antiſceptic; and would it not be abſurd and inconſiſtent with the facts of chemiſtry to ſay, that a portion of matter which had already began to putrefy on being added to an unputrefied maſs ſhould retard the proceſs?

The benefit derived from the uſe of freſh vegetables and their acids in curing ſcurvy, is, in my opinion, by no means to be admitted as proof of the blood's being in a putrid ſtate.

We muſt in this place notice, that it is a well eſtabliſhed opinion, that the powers of digeſtion are ſuch, as to be capable of converting all matters into one and the ſame kind of chyle, whether animal or vegetable, though the one may not afford it in as great proportion as the other.

The above circumſtance being admitted, and it cannot be denied, we at the ſame time muſt allow, that thoſe matters undergo a conſiderable change in the ſtomach before they go on to the formation of the blood. Conſequently, if they ſuffer a change, they poſſeſs no longer the properties of a vegetable, or a vegetable acid, and therefore a tertium quid muſt be formed. What its nature and properties are I will not pretend to ſay, but leave the candid to judge for themſelves; though we might equally well ſuppoſe it to be ſeptic as antiſeptic*.

There are many phyſicians, who ſuppoſed fixed air to be chiefly uſeful when injected into the inteſtines in thoſe diſeaſes ſuſpected to be putrid, by actiug as an antiſeptic on the putrid blood; but when, thus applied, does it not rather act on the contents of the inteſtines, and deſtroy the bad effects produced by their offenſive ſmell, &c. ſince it has been well obſerved, "that, any thing putrid is totally incompatible with the perfect well being of the animal?"

It is generally allowed, that putrid effluvia act as debilitating powers on the ſyſtem. Indeed putrid matters, lying for a time in the inteſtines, would ſeem to be debilitating, as in the inſtance of the dyſentery. It appears alſo that fixed air acts as a ſtimulant: Mr. Henry found that it inflamed an ulcer †. And Dr. Dobſon ſays, that when received by the mouth into the ſtomach, in ten minutes, it raiſed the pulſe from 71 to 77 ſtrokes ‡.

It was objected to me, that the urine, breath, and other excretions of perſons labouring under diſeaſes of the putrid claſs were highly fœtid and obnoxious. We grant this may be the caſe; though a queſtion will then ariſe, whether theſe excretions contract this fœtor from the blood, or become fœtid after they are ſeparated from the general maſs? I am of the latter opinion, and think it highly probable that they become fœtid in their reſpective reſervoirs, or in ſome other manner not yet explained. We know that the excretions do not by any means poſſeſs the properties belonging to the blood, and we alſo know that they may be very different in diſeaſe, though the blood remain the ſame as when in perfect health. To this purpoſe, I will quote Dr. Home's obſervation—he mentions that the blood of diabetic perſons appeared perfectly natural; that the ſerum of the ſame poſſeſſes no more ſweetneſs than that of other blood; though the urine of the ſame patients taſted very ſweet, and upon evaporation afforded a large quantity of ſaccharine matter.*

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* I think that the following circumſtance juſtifies my aſſertion, that vegetable aliment after it has ſuffered the changes produced on food in the ſtomach, &c. when it is ſent on to form the chyle, has more of a ſeptic than antiſeptic quality. Fordyce on Digeſtion, p. 164, mentions, that by diſtillation in a retort we obtain an empyreumatic oil, volatile alkali and water, and charcoal remains in the retort, whether the ſubſtance diſtilled be chyle, a piece of fleſh, or other animal ſubſtance.

† Henry's Exper. p. 127.

† Dobſon's Commentary on Fixed Air.

The caſe of a patient, who was lately under the care of my worthy preceptor Dr. Wiſtar, may alſo be mentioned in confirmation of theſe ſentiments. He was affected with pneumonia, and had all the uſual ſymptoms; but in addition to them, an odour proceeded from him ſo putrid and offenſive that no one could remain long in his room without great inconvenience, it even extended its offenſive ſmell into the rooms a ſtory below him. His urine had alſo an intolerable fœtor. The ſymptoms of pneumonia required bleeding, and this remedy was uſed with great caution; but notwithſtanding the above circumſtances, the blood coagulated very firmly, and had ſome inflammatory appearances on its ſurface.

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* Homes's Clinical Experiments, p. 332.

The arguments taken from Dedier's, Couzier's, and Homes's Experiments, employed by Dr. Ferris to prove, that the bile and blood undergo a change in the plague and meaſles, are by no means concluſive. We know the plague and meaſles to be highly contagious diſeaſes, and eaſily communicated to thoſe who are in a ſtate of prediſpoſition to receive the infection. Dr. Ferris ſuppoſes, that as the blood or bile of one who died of the plague, when injected into the veins of a dog, produced ſymptoms of that diſeaſe; and as the meaſles enſue after inoculating with the blood of thoſe who labour under it, the blood and bile muſt have undergone changes in thoſe diſeaſes. In my opinion theſe facts prove nothing that favours ſuch a concluſion; and all that we can infer from them is, that the contagious or infectious matter was diffuſed through the blood, or adhered to it in thoſe inſtances, as it does to old buildings, cloathing, &c. Facts prove that it does ſo with reſpect to theſe ſubjects, for months, nay even years, and then is as effectual as ever in its deadly operation. We are uninformed of the appearance or ſtate of the blood in thoſe inſtances. In the meaſles, the blood appears to be no more changed or altered than in other inflammatory diſeaſes. How it is in the plague, I know not. Dedier and Couzier have taken no notice of any evident change—Gentlemen whoſe accuracy would not have allowed them to have neglected ſuch a circumſtance if it had occurred.

What confirms me in the opinion, that the contagious matter was only diffuſed through the blood, or adhered to it in the ſame manner as to old buildings, &c. is, that in the one inſtance the plague, in the other the meaſles, was

was produced. If the contagious matter had mixed with the blood ſo as to produce a chemical change therein, neither the plague nor the meaſles would have been the reſult of the Experiments; as by this mixture, they would both have loſt their former properties, and a new compound would have been produced by their union, not poſſeſſing the properties of the contagious matter or of the blood. The ſixth law of the affinity of compoſition, reads thus: "Two or more bodies, united by the affinity of compoſition, form a ſubſtance, whoſe properties are very different from thoſe of any one of the bodies before their combination."* Conſequently a diſeaſe very different from the plague or meaſles muſt be produced, if a chemical union had been formed.†

With reſpect to Dr. Home's Experiment, I beg leave to make one or two obſervations: He ſays, "I thought that I ſhould get the blood more fully ſaturated with what I wanted, if it was taken from the cutaneous veins amongſt the meaſles, than if I took it from a large vein, where there was a much greater proportion of blood from the more internal parts than from the ſkin. I therefore ordered a very ſuperficial inciſion to be made amongſt the thickeſt of the meaſles, and the blood, which came ſlowly away, was received upon ſome cotton."*

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* Fourcroy's Chemiſtry, Vol. I. p. 64.

† We have already proved that the blood in the veſſels of living animals was not ſubject to the laws of fermentation, and that no fermentation could be excited in it by the introduction of ferments; therefore if a change had been produced in the blood in the above inſtances, it muſt have been by a chemical union, and conſequently muſt be ſubject to the laws of chemical affinity.

The Doctor appears to be of the opinion that a fermentation of the blood, produced by the introduction of contagious matter into the ſyſtem, was not the cauſe of the diſeaſe; for, he ſays, the blood taken from the more internal parts was not as plentifully ſaturated with the morbillous matter as that flowing in the cutaneous veſſels. Is not this inconſiſtent with the true and well underſtood courſe of the blood? For, is not the blood of the cutaneous veſſels at one moment in one part, and at another in a very different part, of the ſyſtem? Conſequently the whole maſs muſt have been equally affected. Again, if the motion of the blood were not itſelf ſufficient to produce the change throughout the whole maſs, this would have happened from the well known laws of fermentation; for we know that a very ſmall portion of a ferment is ſufficient to aſſimilate a very large maſs of fermentable matter. Further, this experiment is not concluſive, ſince the blood was obtained by a very ſuperficial inciſion made amongſt the thickeſt of the meaſles, and the blood alſo flowed ſlowly. Here certainly in making the inciſion, the lancet or inſtrument uſed muſt have pierced ſome of the puſtules from the ſituation in which it was made; and as the blood flowed ſlowly it had time to entangle or mix with a quantity of the matter contained in the puſtules; ſo that this experiment is a very indeciſive one. Moreover I have been told by a gentleman who ſometime ſince attended the Lectures of the celebrated John Hunter, that Mr. Hunter informed his pupils, that he had made frequent attempts to inoculate with the blood of thoſe who had the ſmall-pox, and lues venerea, but never ſucceeded in imparting the infection.

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* Home's Medical Facts, &c. p. 268.

Lind's obſervations on the blood in the yellow fever ſupport the opinion, that ſubſtances different from the blood may be diffuſed through it without changing it. He relates that the ſerum was of a deep yellow tinge. A perſon by curioſity taſted it, and found it bitter.*

From theſe facts we learn, that the bile may exiſt in the blood-veſſels, without producing a change in the blood, or ſuffering one itſelf, ſince its ſenſible properties were in theſe caſes evident. Who will aſſert that the properties of the blood are changed in jaundice?

We might proceed to a much greater length in proving that certain ſubſtances may exiſt in the blood-veſſels of living animals unchanged themſelves, and without producing any change in the blood, as turpentine, &c. This, I believe, is a poſition at preſent generally allowed by phyſicians—I ſhall therefore paſs it over in ſilence.

Eruptive diſeaſes were, for ages, ſuppoſed to be owing to changes in the blood; but the following Experiment would ſeem to operate againſt the doctrine. D. Coxe, transfuſed between 14 and 16 ounces of blood from the jugular vein of an old dog, who laboured under an eruptive diſeaſe, into the jugular vein of another dog, who was in health. This being done, the diſeaſed dog got well;

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* Lind's 1ſt. Paper, p. 13.

and to the other, who received the blood which might probably have been ſuppoſed to be affected, nothing amiſs happened.[*]

This Experiment proves clearly that the blood did not ſuffer a change from the diſeaſe under which the dog at that time laboured. Eruptive diſeaſes are, I believe, for the greateſt part contagious; and as the blood did not, in this inſtance, communicate the diſeaſe, we cannot ſuppoſe that the contagious matter adhered to it; this, therefore, ſtrengthens my opinion, and leaves room for the ſuppoſition, that contagion may adhere to different parts of the body in different ſubjects.

If contagious diſeaſes are produced by the contagion operating upon the blood as a ferment, whence is it that the blood of brute animals is not ſuſceptible of being excited into this fermentation, ſince, from Experiments, it appears to be much of the ſame nature with that of the human ſpecies?

Five months after the above ſheets were written, I had the good fortune to obtain a reading of Dr. Milman's very ingenious "Enquiry into the ſource from whence the ſymptoms of the ſcurvy, and of putrid fevers, ariſe, &c." wherein the author diſplays a great deal of ingenious reaſoning. I was happy to find that we agreed in ſentiment reſpecting the general opinion, though we have treated of the ſubject in a very different manner. For particulars I refer the reader to the book itſelf.

Thus I have ſought in nature the phœnomena of my doctrine. In my own eſtimation, facts reſpond to theory,

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[†] Etmulleri Opera, Tom. 3tio, p. 1619.

ory, and the inferences of my Experiments to ſpeculation. When I firſt contemplated this ſubject, I was not particularly attached to any opinion reſpecting it; and, from this circumſtance, I gained the advantage of an impartial and unprejudiced examination of facts. After collating and conſidering the Experiments I had made, I drew my concluſions with a deference to reaſon. If, in the event, I ſhall have contributed to advance the intereſts of Medicine, and furniſhed any principles to aid the labors of practice—to leſſen the horrors of putrid diſeaſes,—and to arreſt, for a moment, the dreadful arm of death, I ſhall be more than compenſated for performing my duty, and paying this tribute to humanity.

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THE END.